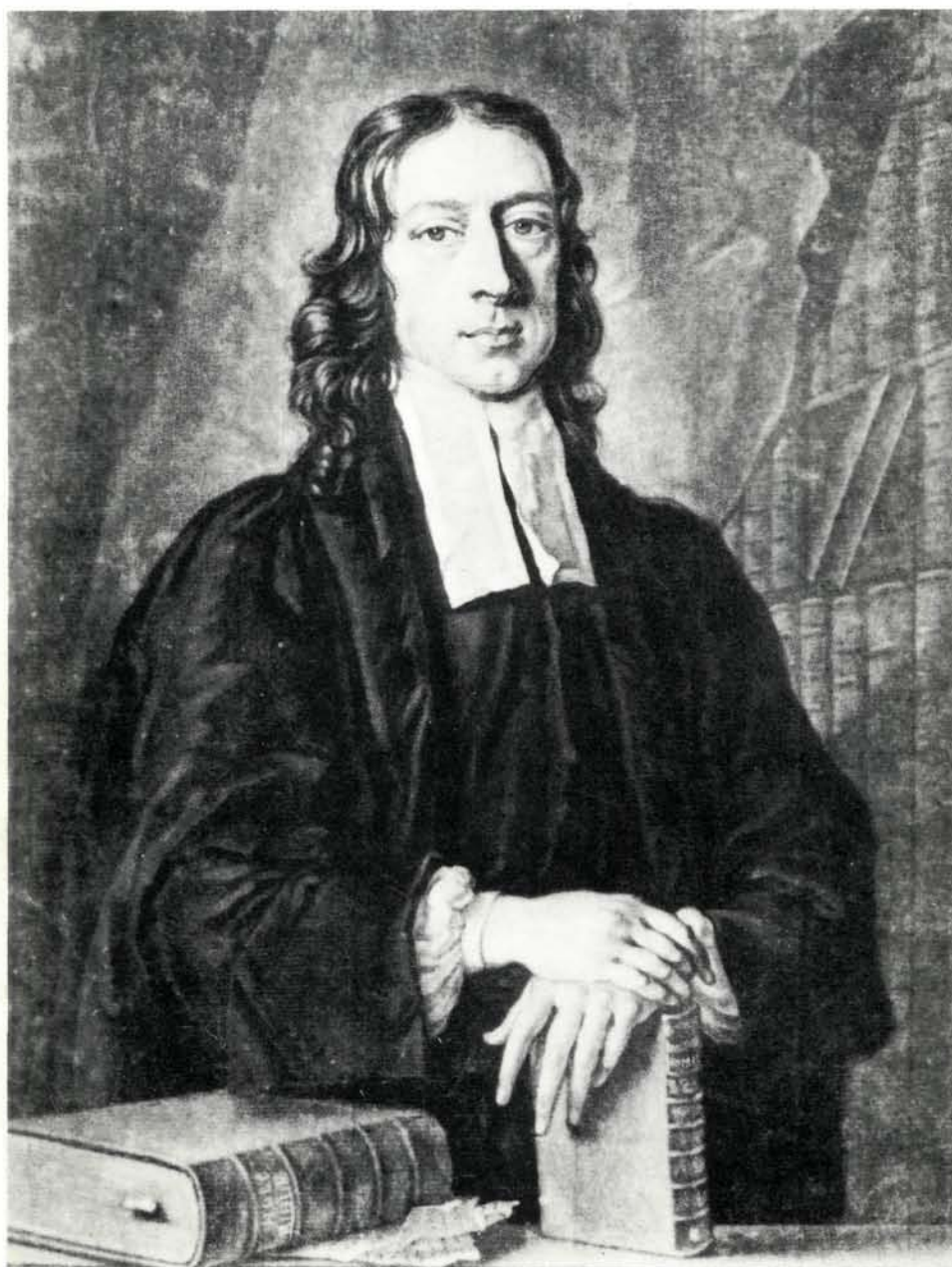


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The Historian and the Study of International Relations

GORDON A. CRAIG

THE CUSTOM OF INVITING DISTINGUISHED FOREIGN SCHOLARS to become honorary members of the American Historical Association goes back to the year 1885; and the first of them were Leopold von Ranke, William Stubbs, Samuel Rawson Gardiner, and Theodor Mommsen. This was a formidable quartet, and their names are still capable of causing an involuntary nod of respect when they are mentioned; but I should be surprised to learn that their works were much read now. The importance of reading the older works even in one's special field seems to have diminished as a result of the great expectations of new discoveries engendered by the invention of scientific techniques for exploiting the archives;¹ and, as for general reading, so many books roll off the presses every year that it is easy to conclude that there is no time for reading the classics, which are, in any case, old-fashioned and out of date.

This prejudice, for that is what it is, sometimes makes our critical judgments unbalanced, if not naive. It was surprising to find, in the special issue of the *Journal of Modern History* devoted to Fernand Braudel's *The Mediterranean*,² no mention of Mommsen's *Roman History*, although it too dealt with the Mediterranean as a physical and human unit and discussed such questions as overlapping civilizations, the Jews as a separate civilization, and the extension of Mediterranean culture to the Western world, and although it included descriptions of religion, money, industry, agriculture, the institutions and economy of war, architecture, art and literature, public morality, crime, cuisine, and other aspects of Mediterranean life from Egypt to Spain that are as rich as, and perhaps more disciplined than, Braudel's.³

This presidential address was delivered at the Ninety-Seventh Annual Meeting of the American Historical Association, held in Washington, D.C., December 28–30, 1982.

¹ See H. Stuart Hughes, "Contemporary Historiography: Progress, Paradigms, and the Regression toward Positivism," in Gabriel A. Almond *et al.*, eds., *Progress and Its Discontents* (Berkeley and Los Angeles, 1982), 240–48. On some negative effects of reliance on such techniques, see Konrad Repgen, "Methoden- oder Richtungskämpfe in der deutschen Geschichtswissenschaft," *Geschichte in Wissenschaft und Unterricht*, no. 10 (1979): 603.

² "History with a French Accent," *Journal of Modern History*, 44 (1972): 447–539.

³ Mommsen, *Römische Geschichte* (1854–56, 1885), 8 vols. (reprint edn., Munich, 1976), esp. 1: 466–69, 2: 321, 356–89, 3: 275–315, 4: 291, 5: 178–217, 232–34, 6: 158–79, 230–94, and 7: 188–248.

Our first four honorary members were political historians, and all were concerned with the State: Stubbs with the genesis and growth of its institutions, Gardiner with its travails in time of civil war (but also with its foreign relations), Ranke with its nature and its role in the world of States, and Mommsen with its growth to world power and the effect that that had upon its culture and institutions. This preoccupation may be another reason for their relative neglect. In recent years political history has not been the liveliest of fields—ten years ago, Jacques Le Goff described it as “a corpse that has to be made to lie down”⁴—and diplomatic history in particular has failed to engage the attention of the profession, whose prevailing attitude has varied between condescension and antipathy. The chief reason for this aversion is probably a pervasive feeling that its methods are outmoded and that, in comparison with some of the newer fields of specialization, it is unlikely to yield anything very new in the way of results, although what Charles S. Maier has called “a bad conscience about the legitimacy of American power”⁵ since Vietnam has, no doubt, also made the study of the relationships and rivalries and conflicts of the Great Powers distasteful to many scholars.

The decline of the study of international relations is, in any case, palpable. It is reflected in the diminished attention given to political and diplomatic history in professional and scholarly journals (in 1970, when *Daedalus* planned an issue on “The Historian and the World of the Twentieth Century,” the decision to include an article on political and diplomatic history was very belated), in its modest representation in the programs of the annual meetings of this Association (for the years 1976–82 inclusive, the study of international relations very broadly conceived has averaged 5 sessions out of 128), and in its shrunken share in university history departments and curricula. The great luminaries of our profession are no longer diplomatic historians, as they were in the 1930s and 1940s and even the 1950s, and some departments have none at all.

This trend confronts us with a paradoxical situation. There have been, depending on how one counts, five wars in the Far East since 1945, six in the Middle East, one in the South Atlantic, and any number of bloody conflicts in Africa. The world we live in is just as filled as it was in the 1930s with combustible materials, and their potential for destroying us if we don’t bend our collective intelligence toward preventing annihilation is infinitely greater. And yet, in the face of this harsh truth, our interest in, and commitment to, the study of international relations has shown no sign of increasing.

It is hard to justify this or to believe that it does not represent a disservice to the lay audience from which we ultimately derive our legitimacy.⁶ That audience is interested in foreign affairs, as can be seen from many signs and portents, ranging from its continuing fascination with diplomatic memoirs to the nuclear freeze movement, and not excluding the revival, on some campuses, of international

⁴ Goff, “Is Politics Still the Backbone of History?” in Felix Gilbert and Stephen R. Graubard, eds., *Historical Studies Today* (New York, 1972), 348.

⁵ Maier, “Marking Time: The Historiography of International Relations,” in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 356.

⁶ H. R. Trevor-Roper, *History: Professional and Lay*, Inaugural Lecture Delivered on November 12, 1957 (Oxford, 1957).

relations programs outside and at the expense of history departments. The general public has a right to feel that our work should bear some relevance to its concerns, to expect the historian to do what Friedrich Schiller, in his inaugural lecture at Jena in 1789, said he should do—namely, “select from the stream of events those that exercise an essential, unmistakable, and easily comprehensible influence on the *present* shape of the world and the situation of the contemporary generation.”⁷ Unless we are prepared to ignore that feeling, and to close our ears to poet Roy Fuller’s warning that

The treason of clerks is when
They make a fetish of the pen,
Forget that art has duties to—
As well as to the “I”—the “You,”
And that its source must always be
What presses most, most constantly,

then we should perhaps think about directing more attention and a greater proportion of our resources to what has become—incongruously, given the state of the world—a neglected field.

EVEN DURING ITS DECLINE, the historical study of international relations has grown in scope and sophistication. We are a long way from the time when the standard monograph in diplomatic history was literally copied out of the bound volumes of the Foreign Office papers in the Public Record Office, tricked out with Latin tags and formidably arcane footnotes, and set forth to grace the lower shelves of university libraries. In recent years, diplomatic history has embraced more general questions, like the moral and intellectual roots and assumptions of national policy, domestic factors as determinants of policy, interagency competition in decision making, public opinion and the way in which it is influenced by the media, comparative political systems and ideological convergence, and much else. This broad scope is commendable but, like many good things, has tended to become excessive and to lead to a kind of reductionism in which the State as an independent actor has disappeared and diplomatic history has been subsumed under social history. German historians, for example, inspired by Thomas S. Kuhn’s book on scientific revolutions,⁸ have for some time been arguing that traditional paradigms like the national state and the concepts of hegemony and balance are no longer satisfactory and that the great movements of modern politics must be regarded as functions of the process of modern industrialism.

Works of this sort, and the varied attempts to assert a *Primat der Innenpolitik*, have been less than satisfactory. The impressive amount of scholarship devoted to structural explanations of German foreign policy before 1914, for example, have succeeded at best in giving an undifferentiated and static account that fails to

⁷ Schiller, “Was heisst und zu welchem Ende studiert man Universalgeschichte?” in *Schillers Werke*, 6 (Munich, 1912): 270.

⁸ Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962).

explain why particular decisions were made, rather than quite different ones, and why they had the results they had.⁹ Such things, it becomes ever clearer, cannot be explained without analysis of the international system and its dynamics. Or, for that matter, without reference to something that Ranke understood but that we have either forgotten or sought to depreciate—namely, the autonomy of the State and its tendency to go its own way and resist pressures upon it.

Ranke put this in terms that we would doubtless reject as mystagogical when he talked of States as individuals with their own lives, “progressing amid all the turmoil of the world . . . , each in its own way, . . . celestial bodies, in their cycles, their mutual gravitation, their systems!”¹⁰ But Eric A. Nordlinger has said very much the same thing in a recent book in which he has rejected the society-centered perspective that, he said, has “a pervasive grip upon citizens, journalists and scholars alike,” and has argued persuasively that even the democratic State “is not only frequently autonomous insofar as it regularly acts upon its preferences, but also markedly autonomous in doing so even when its preferences diverge from the demands of the most powerful groups in civil society.”¹¹

How the State asserts its authority in foreign affairs has been described by Stanley Hoffmann in a passage that emphasizes the degree to which its sphere of action is composed not of determinable but of uncertain factors that it is the duty of statecraft to assess, shape, and exploit. Statecraft, Hoffmann has claimed,

emanates from a milieu—the domestic society—whose values, political and social institutions, experiences, and patterns of authority are never entirely fixed or coherent, never point only in one direction, and, while ruling out certain choices, leave a considerable margin for maneuver . . . ; and statecraft operates in a milieu—the international system—that has repeatedly been defined as an arena for competition for multiple stakes, with uncertain rules which the players . . . hammer out by trial and error, and characterized by moves which, however cleverly calculated, are more like wagers than rational adaptations of means to ends.¹²

In this realm of ambiguity, the statesman must ask himself repeatedly, How much choice do I actually have? How compelling are the domestic and foreign considerations that I must bear in mind? How much freedom do I derive from, or to what extent am I limited by, the stability and effectiveness, or the unsteadiness and incompetence, of my political system compared with my opponent's, our relative physical and moral resources, and the momentum of events? And he must at the same time remember that the game does not end when he makes up his mind to act or not to act, for once decisions are implemented they assume a life of their own, producing reactions and counterreactions among the other players and creating situations that may confound original expectations. The decision of the German

⁹ On such structural explanations, see Klaus Hildebrand, “Geschichte oder ‘Gesellschaftsgeschichte’: Die Notwendigkeit einer politischen Geschichtsschreibung der internationalen Beziehungen,” *Historische Zeitschrift*, 223 (1976): 328–57; and Paul Kennedy, “The Kaiser and German *Weltpolitik*: Reflections on Wilhelm II's Place in the Making of German Foreign Policy,” in John C. G. Röhl and Nicolaus Sombart, eds., *Kaiser Wilhelm II: New Interpretations* (Cambridge, 1982), 143–48.

¹⁰ Ranke, “A Dialogue on Politics” (1836), in Theodore H. Von Laue, *Leopold von Ranke: The Formative Years* (Princeton, 1950), 168, 180.

¹¹ Nordlinger, *On the Autonomy of the Democratic State* (Cambridge, Mass., 1981), 1, 7, 203–19.

¹² Hoffmann, *Gulliver's Troubles: or, The Setting of American Foreign Policy* (New York, 1968), xvi.

government in 1890 not to renew its alliance with Russia was intended to give greater coherence to the German alliance system and to encourage the British to join it. It had quite the opposite effect, for Germany's junior partners were tempted to raise the price of their collaboration, while the British, no longer having to worry about coordinated pressures from Berlin and Petersburg, became more aloof. Disconcerted, the German government, after what Ambassador Paul von Hatzfeldt called a period of "hysterical vacillation,"¹³ tried to regain the initiative by a policy of colonial blackmail, which elicited rather firmer responses than it had expected and further contributed to the deterioration of Germany's position.

It is these aspects of international relations to which—if we can only moderate our absorptive interest in the domestic influences on policy—we should direct our attention: the story of how nations deal with each other, their actions in specific cases, the modalities they employ and the combinations they form in order to protect and advance their interests, their disputes and the ways in which they are or are not resolved, and the ways they get in and out of wars.

IN A SADLY NEGLECTED BOOK, D. P. Heatley wrote in 1919 that, before the diplomatic historian embarks upon his special research, he must try to acquire "the habit of mind that is required for appreciating questions of foreign policy," and he went on to say, "We must never separate the study of policy . . . from the appreciation of the instruments on the understanding and use of which success depends, and we must test the character of the instruments by the work they have to do."¹⁴ Heatley thought it unlikely that historians could acquire all this from practical experience but suggested that they could at least profit from reading in the specialized literature on the art and technique of negotiation. This remains good advice. Indeed, the great handbooks on diplomacy from Wicquefort and Callières to Martens and Satow are an indispensable source for the writer on international relations, not merely for technical knowledge but for a sense of the continuity of problems in foreign affairs, the limitations set on policy, and the very feel of the diplomatic process.¹⁵ The neglect of this literature has contributed to the foreshortening and one-dimensionality of much recent diplomatic history.¹⁶

The contemporaneity of these works is startling. Consider the remarkable memoirs of the Flemish soldier-diplomat Philippe de Commines, who served both Charles the Bold of Burgundy and Louis XI of France, accompanied Charles VIII during his invasion of Italy in 1495, and went on two missions to Venice to try to forestall the formation of the Sainte Ligue that turned that adventure into a fiasco. Commines, though not a very successful diplomat, was observant, and his memoirs

¹³ Hermann von Eckardstein, *Lebenserinnerungen und politische Denkwürdigkeiten*, 2 (Leipzig, 1920): 161.

¹⁴ Heatley, *Diplomacy and the Study of International Relations* (Oxford, 1919), 86–87, 4–5.

¹⁵ Wicquefort, *L'Ambassadeur et ses fonctions par Monsieur de Wicquefort, Conseiller aux Conseils d'Etat et Privé du Duc de Brunswick et Luneburg Zelle, etc.* (Cologne, 1690); Callières, *On the Manner of Negotiating with Princes by Monsieur de Callières* (1716), trans. A. F. Whyte (1919; reprint edn., South Bend, Ind., 1963); Charles de Martens, *Le Guide diplomatique: Précis des droits et des fonctions des agents diplomatiques et consulaires*, ed. M. F. H. Geffcken, 2 vols. in 3 (Leipzig, 1866); and Ernest Satow, *A Guide to Diplomatic Practice*, 2 vols. (London, 1917).

¹⁶ Gordon A. Craig, "On the Nature of Diplomatic History: The Relevance of Some Old Books," in Paul Gordon Lauren, ed., *Diplomacy: New Approaches in History, Theory, and Policy* (New York, 1979), 21–42.

are filled with incisive, if melancholy, reflections on the politics of his day and the art of diplomacy that are not without a piquant modern relevance, like his repeated insistence upon the predictably disastrous effects of summit conferences.¹⁷ Particularly striking is his conviction that the anarchic individualism of Europe's rulers, imperfectly restrained by the fear of God's wrath, might be curbed (and this is suggested rather than said) if men built upon the rudimentary elements of equilibrium that the state system provided. These he described, quaintly enough, by saying that God had given to every nation

quelque aquillon. Car au royaume de France a donné pour opposite les Angloys; aux Angloys a donné les Escossoys; au royaume d'Espagne Portugal. ... Il pourroit donques sembler que ces divisions fussent necessaires pour le monde et que ces esquillons et choses opposites ... sont necessaires ... et principalement pour la bestialité de plusieurs princes et aussi pour la mauvaistié d'autres qui ont sens assez et experience, mais ilz en veulent mal user.¹⁸

For the political mores of his time, Commynes had a deep aversion, and he saw nothing glorious in the age's continuous warfare, regarding the new weaponry, the artillery that Charles VIII brought into Italy, with the same repugnance as his contemporary Ludovico Ariosto, who called it a

hellish instrument . . .
the worst device, in all the years
of the inventiveness of humankind,
which e'er imagined was by evil mind.¹⁹

More clearly than any of his contemporaries, Commynes realized what was at stake in the unremitting competition of the European states. He saw that these rivals were dependent upon each other whether they liked it or not ("Car nulle mutation ne peult estre en ung royaulme qui ne soit douloureuse pour la pluspart; et combien que aucuns y gaignent, encore il y eu a cent foiz pluz qui y perdent");²⁰ he feared that their tendency toward "bestiality" would destroy them all if it continued to be uncontained.

These concerns are still our concerns, and the attempts, since Commynes's time, to devise means to restrain international violence and check the hegemonial ambition of single powers have been among the great themes of modern diplomatic history. Since A. H. L. Heeren's *History of the Political System of Europe* appeared in 1809 and Ranke's essay *The Great Powers* in 1833,²¹ efforts to devise viable international systems have intrigued the historical imagination, and in this century a long line of distinguished practitioners—one thinks of Webster, Temperley, and Sumner, of Renouvin, Woodward, Taylor, and Medlicott, of Chabod, Langer, and Sontag—have written of the emergence and elaboration of the nineteenth-century system, its working assumptions and operating rules, its mutations and transforma-

¹⁷ Commynes, *Mémoires*, ed. Joseph Calmette, 3 vols. (Paris, 1924), 1: 87, 135, 138–39, 141.

¹⁸ *Ibid.*, 2: 208, 211.

¹⁹ Ariosto, *Orlando Furioso*, trans. Barbara Reynolds, 1 (Baltimore, 1975): 350–51.

²⁰ Commynes, *Mémoires*, 3: 299–300. Compare Callières: "No considerable change can take place in any one [of the states of Europe] without affecting the condition, or disturbing the peace, of all the others"; *On the Manner of Negotiating with Princes*, 11.

²¹ Heeren, *Handbuch der Geschichte des europäischen Systems und seinen Kolonien* (Göttingen, 1809). For a translation of Ranke's *Die Grossen Mächte*, see Laue, *Leopold von Ranke: The Formative Years*, 181–218.

tions, and its eventual collapse. The time has now come to extend that investigation to the attempts at system-building in our own time, to the effects of the expansion of the international community and the breakdown of its ideological homogeneity, to bipolar and tripolar systems, to systems that exist on the basis not of agreement but of tacitly defined disagreement (the Cold War, for example, as a political system), and to the changed relationship between force and statecraft, which has diminished the reliability of the former as an instrument of the latter and led to the paradoxical situation that military force is now useful only as long as it is not used.²²

Even the best of the older diplomatic histories tended to be descriptive rather than analytical, particularly when dealing with the modalities that supported systems, like alliances, the crises that threatened them, and even the perennial process of negotiation. Roger V. Dingman has pointed out with respect to alliances that for any very extensive discussion of the unstable combinations of passion and reason and fear that inspire and sustain them and of the way they actually work, one must return to Thucydides, particularly to his discussion of the debate between Corcyra and Corinth over the former's request for an alliance with Athens and his account of the unhappy course of the alliance between Athens and Mytilene.²³ Modern historians have often been imprecise in their terminology, making no distinction between alliances, alignments, ententes, and coalitions and—unlike Thucydides—generally regarding alliances from a legal or operational point of view rather than seeing them as quasi-organisms, composed of complex linkages and existing in a multilayered environment that itself changes over time. Dingman has suggested that diplomatic history would profit from a more analytical and differentiated approach that would pay attention to the nature of the binding elements between partners (whether the alliance is sustained by ties between individuals, political agencies or military services, private agencies, or peoples), to the effect of changes of leadership (like the fundamental change in Anglo-American relations when John Foster Dulles succeeded Dean Acheson as secretary of state), and to the inner dynamics of change that operate in long-term alliances and confront the alliance managers periodically with the necessity of making adjustments, perhaps in purpose and structure, if the alliance is to retain its vitality.²⁴ This seems reasonable enough. Indeed, it cannot help but be instructive to restudy the history of older alliances in the light of the life cycle of the NATO alliance, which itself validates the usefulness of Dingman's analytical scheme.

Much of what has been said of alliances may also be applied to crises, the purple passages of diplomatic history, exciting recent examples of which await the pen of the historian. The fact that, for the post-1945 period, all of the documents are not available and that the Soviet diplomatic records are probably going to remain so should not discourage historians from tackling these subjects, and Robert M. Slusser, in his study of the Berlin crisis of 1961, has shown what can be accomplished by the artful use of memoirs, official releases, and press coverage.²⁵

²² Richard J. Barnet, "The Search for National Security," *New Yorker*, April 27, 1981, esp. pp. 105–34.

²³ Thucydides, *The Peloponnesian War*, trans. Rex Warner (Baltimore, 1972), esp. 54–67, 197–200.

²⁴ Dingman, "Theories of, and Approaches to, Alliance Politics," in Lauren, *Diplomacy: New Approaches*, 245–66.

²⁵ Slusser, *The Berlin Crisis of 1961* (Baltimore, 1973).

The Slusser study suggests the need for an investigation of the broader complex of which the Berlin crisis was a part—the whole sequence of events that began with the Khrushchev ultimatum on Berlin in November 1958 and ended with the Cuban missile crisis of 1962.²⁶ In retrospect, this appears as a series of linked crises of mounting intensity, which were generated, on the one hand, by Soviet concern over unrest in the satellite states and ideological sniping from Peking and, on the other, by changing Soviet perceptions of American leadership; and its final resolution proved to be the turning point from the Cold War to the detente period, reminding us that the Chinese character for crisis also means opportunity.²⁷ It is possible that crises, like alliances, should be regarded metaphorically as organisms whose nature changes in response to mutations in the international environment and that pass through a perhaps predictable sequence of transitions—that there is, in fact, an anatomy of crisis that it would be useful, for general purposes of historical analysis, to discover. It is clear, moreover, that the catalytic effects of crisis deserve more systematic attention than they have received.

As for negotiation, it is perhaps enough to say that, although the basic principles of bargaining and compromise are much what they were in the classical age of diplomacy,²⁸ its forms and procedures have been modified as a result of the increased heterogeneity and expansion of the international community and of the revolution in communications, the increased influence of public opinion, and even the nature of military power in our time. Diplomatic historians have normally thought of negotiation as the province of the individual statesman, generally representing a Great Power, a Kaunitz or a Castlereagh, guided by the principles of *Staatsräson* and by a jealous regard for his country's interests, who masters the complexities of the issues at stake, confounds his rivals by his dialectical brilliance, and finally brings them to his way of thinking. *Pace* Henry Kissinger, this model fits few of the negotiating situations of our day. In the Conference on Security and Cooperation in Europe (CSCE), for example, the chief of the U.S. delegation did not play a central role in developing or even have any influence on the American negotiating positions, which were prepared in Washington, in the National Security Council and the Departments of State, Defense, Commerce, and the Treasury, which intermittently sent experts to advise the delegation. Before the conference was over, its sessions had been attended by a number of congressmen, and it was being examined by several congressional subcommittees. This bureaucratization of the negotiating process left little room for diplomatic virtuosity or, for that matter, since the recommendations of the various agencies were not always perfectly coordinated, for a clear conception of the national interest.

Notable also at the CSCE was the heightened role of the lesser powers, not only in pressing successfully for the conference (against the wishes, in the first instance, of the U.S. government) but in sharing on equal terms in all decisions. In a way that

²⁶ Slusser himself has made a start on this: "The Berlin Crises of 1958–59 and 1961," in Barry M. Blechman and Stephen S. Kaplan, eds., *Force without War: The U.S. Armed Forces as a Political Instrument* (Washington, 1978), 343–439, which is, however, largely a rather speculative account of Soviet policy that is very sparse on the policies of other powers. Also see Jack M. Schick, *The Berlin Crisis, 1958–1962* (Philadelphia, 1971), an excellent chronological account but analytically cautious.

²⁷ Gordon A. Craig and Alexander L. George, *Force and Statecraft: Diplomatic Problems of Our Time* (New York, 1983).

²⁸ See Fred C. Iklé, *How Nations Negotiate* (New York, 1964).

would have been considered unthinkable either at Vienna in 1814–15 or at Paris in 1919, all thirty-five delegations participated in the negotiation of all parts of what became the Helsinki Agreement of 1975, and all decisions were made by consensus. This process of reaching accord may indicate that the reliance of the superpowers upon arsenals of nuclear weapons that threaten the whole community has increased the determination of the lesser powers to resist superpower dictation and insist upon the right to be consulted on all decisions that may affect peace or war, and that this new resolve may have profound effects on diplomacy in other contexts.²⁹ In any event, it is clear, from the Middle East crisis of 1973 and the Lebanon crisis of 1982, that the diplomatic historian is going to have to be prepared to deal with increasingly complicated forms of negotiation.

IN DEALING WITH THESE AND OTHER PROBLEMS of recent diplomacy, we may gain in analytical sophistication if we overcome our congenital distrust of theory and our insistence upon the uniqueness of the historical event. Thucydides once wrote of “events which happened in the past and which (human nature being what it is) will, at some time or other and in much the same ways, be repeated in the future”;³⁰ and in this spirit some of our colleagues in political science have reminded us that one can, after all, on the basis of similarity, treat unique cases as members of a class or type of phenomenon and, by appropriate methods of analysis, discover correlations among different variables that may have causal significance or, at the very least, serve as indicators of predictive value.³¹ By the use of case studies and what has been called the method of structured focused comparison, Alexander L. George and Richard Smoke have described the various ways in which deterrence has been used in U.S. foreign policy and the combinations of technique and circumstance that have made for success or failure in its employment, elaborating in the course of their analysis a theory of deterrence;³² and there is no good reason why this method should not be applied to the study of a whole range of diplomatic modalities and issues.

This enterprise can best be conducted by means of a collaboration between disciplines, to the benefit of both. Political scientists would profit from the fidelity to *milieu et moment* that historians would bring to case studies; they, in turn, might learn from the analytical techniques employed by their partners some new questions to ask in their individual research and some new ways to test the validity of their hypotheses. And, at the very least, their comparative sense would be quickened.

The future historian of the Nixon-Kissinger detente policy, for example, will certainly be struck by Kissinger's explanation of its failure. In the second volume of his memoirs, he argued that, since “détente is the mitigation of conflict among adversaries, not the cultivation of friendship,” it was particularly hard for Americans to understand. “The American perception of international affairs,” he wrote, “has traditionally been Manichean. Relations among states are either peaceful or

²⁹ On the CSCE and its consequences, see Craig and George, *Force and Statecraft*, 166–71.

³⁰ Thucydides, *Peloponnesian War*, 48.

³¹ Alexander L. George, “Case Studies and Theory Development: The Method of Structured Focused Comparison,” in Lauren, *Diplomacy: New Approaches*, 43–68.

³² George and Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York, 1974).

warlike—there is no comfortable in-between.” Kissinger hoped to change this: “We in the Nixon Administration . . . felt that our challenge was to educate the American people in the requirements of the balance of power,” but Watergate made this impossible.³³

There is, of course, some truth in Kissinger’s position. But, if one takes the trouble to study the terminology of the older diplomacy, one discovers that *detente* was generally understood to mean only the first stage in the process of mitigation of conflict. It was no more than an easing of tension, which might be temporary or might, in the right circumstances, lead to a rapprochement between the powers in question. That, in turn, might or might not change their relationship into an *entente*, which might finally, if all went well, eventuate in a process of appeasement to remove salient differences between them, or even in an alliance. These terms were carefully differentiated and regarded as distinct stages in a difficult process, and transition from one to another was dependent upon clearly understood conditions and changes of climate.³⁴

A careful comparative analysis of the origins of the Anglo-French *entente* of 1904 and its subsequent development into an alliance, the appeasement policy of Neville Chamberlain, the *Ostpolitik* of Willy Brandt, and the Nixon-Kissinger *detente* policy indicates that both Chamberlain and Kissinger (the former certainly more rashly than the latter) anticipated the end result of what should have been a laborious process, without—and this contrasts with the measured approach of the British and French governments at the beginning of the century and of Willy Brandt when he turned east in 1970—proper attention to antecedent conditions. This suggests that the ultimate failure of the Kissinger policy of *detente* may have been due less to the reasons that he has adduced than to a not unjustified perception in this country that the Soviets were being given rewards that they had not earned, because there was no persuasive evidence that they had modified their behavior, although that was the point of the exercise.³⁵

Despite the advantages that collaboration with the political scientists can bring to the diplomatic historian, they should not, of course, be his principal concern. He has more than enough work of his own to do; not only is the history of the period since 1945 ripe for investigation, as John Lewis Gaddis has just shown in his masterful and comprehensive account of the evolution of containment policy from Kennan to Carter,³⁶ but many subjects that one might have thought to be finished and filed away are in reality waiting to be reawakened to new life by insight and imagination and a sense of relevance. These attributes inform Samuel R. Williamson’s meticulous account of the origins of the Anglo-French alliance of 1914³⁷ and Paul Kennedy’s study of the rise of Anglo-German antagonism in the last part of the nineteenth century, which combines incisive analysis of the changing context of

³³ Henry L. Kissinger, *Years of Upheaval* (Boston, 1982), 50, 753, 980–85.

³⁴ See Lord Haldane’s distinction, in conversation with Paul Cambon in 1912, between *detente* and *entente*, in G. P. Gooch and H. W. Temperley, eds., *British Documents on the Origins of the War*, 6 (London, 1930): no. 506 p. 681.

³⁵ Craig and George, *Force and Statecraft*, chaps. 10, 17.

³⁶ Gaddis, *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy* (New York, 1982).

³⁷ Williamson, *The Politics of Grand Strategy: Britain and France Prepare for War, 1904–1914* (Cambridge, Mass., 1969).

international relations with a powerful narrative account of the role of individuals and elites in the decision-making process.³⁸ Works like these remind us that history is something more than a social science, intent upon structures and broad lines of development, that it must, as Ernst Schulin has written, “place an extraordinarily high valuation on the significance of the particular momentary situation, the concrete desires and decisions of individual persons, the surprising ‘chance’ events. No other discipline can take from it the task of balancing these great factors. It is precisely for this purpose that it is there.”³⁹

THERE REMAINS, particularly if we wish to regain our credit with the lay audience, the question of style, which has, unfortunately, been one of the chief casualties of the increasing specialization of our discipline. Some time ago, when the Council was discussing the desirability of founding a popular journal of history, it received a memorandum suggesting that it might be difficult to find writers for it, since there were now “few historians . . . who [could] speak to anybody except in the jargon of their specialty.” The writer continued, “The profession must be taught to divest itself of the literature of pedantry, obscurity and boredom and to realize that . . . its economic survival may well depend upon an ability to communicate with those who support, with their tuition and endowments, our centers of scholarly research.”

If there is any merit in this, we should be well advised to take as models for emulation our first four honorary members. Stubbs wrote in a clear and vigorous prose that at times rises to eloquence. Gardiner’s style, if not graceful, is never prolix; he had the gift of sympathy and understanding when judging the virtues and the weaknesses of complicated persons like Strafford and Cromwell, and his accounts of military and naval warfare are clear and often stirring. Ranke’s *History of the Popes*, which contains some of his most penetrating observations on foreign affairs, has been described as “not only a great achievement of historical research but a perfect work of art.”⁴⁰ And Mommsen’s *Roman History*, which fulfilled its author’s desire “to make the ancients step out of their fantastic cothurnus and bring them into the real world for the reading public,”⁴¹ accomplished this objective without falsification or oversimplification; Mommsen indeed exemplified the qualities that he most admired in Polybius: “Truthfulness was second nature to him; . . . his eye was ever directed to the actual course of events; . . . [and] the manner of telling was a model of comprehension, simplicity, and clarity.”⁴²

It was an unknown Roman, one of Mommsen’s anonymous characters, who first uttered the words that in time became proverbial, *Stilus virum arguit* (“The style proclaims the man”). The saying is no less true when amended to read, *Stilus rerum scriptorem arguit* (“The style proclaims the historian”). It is to be hoped that the future historians of international relations, who will have so many things to remember and think about, will not be unmindful of this maxim.

³⁸ Kennedy, *The Rise of Anglo-German Antagonism, 1860–1914* (London, 1980).

³⁹ Schulin, “Die Frage nach der Zukunft,” in G. Schulz, ed., *Geschichte heute. Positionen, Tendenzen, und Probleme* (Göttingen, 1973), 133.

⁴⁰ G. P. Gooch, *History and Historians in the Nineteenth Century* (new impression, London, 1935), 87.

⁴¹ Alfred Heuss, *Theodor Mommsen und das 19. Jahrhundert* (Kiel, 1956), 64.

⁴² Mommsen, *Römische Geschichte*, 3: 465.

Faith and Experience in the Thought of John Wesley

FREDERICK DREYER

TO EXAMINE WHAT JOHN WESLEY THOUGHT is to do something many historians may dismiss as futile. Methodism figures in textbooks not as an idea but as a mood or a state of mind. The typical Methodist is regarded as someone who feels much and thinks little. E. P. Thompson in *The Making of the English Working Class* identified Methodism as “a ritualised form of psychic masturbation.” For Thompson, Methodism is intelligible as a social or psychiatric phenomenon but not as a system of thought. This refusal to consider John Wesley as a man with something to say is not new. Thompson has merely helped perpetuate an established historical prejudice. For Leslie Stephen, Wesley was a man deficient in “speculative insight.” His gifts were practical, not intellectual. He could stir up feeling. “For the immediate purpose of stirring the stagnating currents of religious emotion, no man could have been more admirably endowed.” The emotions that Wesley aroused, however, had nothing to do with thought. Methodism in Stephen’s words was merely “heat without light—a blind protest of the masses, and a vague feeling after some satisfaction to the instinct.”¹

Stephen’s contempt for Methodist thought was fully shared by his great contemporary William Edward Hartpole Lecky. For Lecky as for Stephen, Methodism succeeded because of its emotional appeal: “it satisfied some of the strongest and most enduring wants of our nature.” It possessed no intellectual dimension that was worth our while to study. “The Methodist movement,” Lecky wrote, “was a purely religious one. All explanations which ascribe it to . . . merely intellectual causes are at variance with the facts of the case.” Indeed, for Lecky, Methodism owed its success to the very deficiencies of Wesley’s intellect. “If Wesley had not been very credulous and very dogmatic, utterly incapable of a suspended judgement, and utterly insensible to some of the highest intellectual tendencies of his

On September 13, 1982, I read a shortened version of this essay to a meeting of the Canadian Methodist Historical Society. The Social Sciences and Humanities Research Council of Canada supported my research with a leave-fellowship in 1980–81. I am also indebted to Roger Emerson for his guidance and advice on philosophical questions and to the staff of the United Church of Canada Committee on Archives and History and of Emmanuel College Library for help in my use of their magnificent Wesley collection.

¹ Thompson, *The Making of the English Working Class* (New York, 1966), 368; and Stephen, *History of English Thought in the Eighteenth Century*, 2 (1902; reprint edn., London, 1962): 348, 360–61.

time, it may be safely asserted that his work would have been far less." This tendency to dismiss Wesley's thought is not confined simply to hostile commentators like Lecky, Stephen, and Thompson. Luke Tyerman—himself a Methodist minister, the author of the standard Victorian life of Wesley, and a man who was second to none in his admiration of Wesley's merits—hesitated to claim anything for Wesley as a thinker. In his biography of Wesley, Tyerman explicitly shunned all consideration of "what may be called the *philosophy* of Wesley's life." Tyerman's Wesley was a man with one simple idea, to save souls. "This was the philosophy of his life. All his actions had reference to this. . . . The man is best known by what he *did*; not by what philosophers may suspect he *thought*."²

From one point of view, Wesley has only himself to blame if posterity dismisses him as a thinker or supposes that what he thought is not very important to the explanation of what he did. He often affected a kind of simplicity and plainness of statement that can easily mislead the uncritical reader. In the preface to his edition of the standard sermons, he appeared to scorn philosophy: "I design plain truth for plain people: . . . of set purpose, I abstain from all nice and philosophical speculations; from all perplexed and intricate reasonings; and as far as possible, from even the show of learning." Here and elsewhere he spoke of himself as *homo unius libri*.³ With pride he recalled that he was sometimes denounced as a Bible-bigot or a Bible-moth.⁴ He believed in the inerrancy of scripture and sometimes spoke like a fundamentalist who cared to understand nothing but the word of God. "In matters of religion I regard no writings but the inspired." In the interpretation of the word of God, he condemned "curious metaphysical disquisition about what God has not plainly revealed." To a colleague who attempted to explain the nature of the Trinity by the use of reason, Wesley objected, "What have you or I to do with that difficulty? I dare not, will not, *reason* about it for a moment. I believe just what is revealed, and no more. But I do not pretend to *account* for it, or to solve the difficulties that may attend it. Let angels do this, if they can. But I think they cannot."⁵

The problem of defining what Wesley thought is complicated, furthermore, by the elusiveness of Methodism as a formal creed. Wesley repeatedly refused to identify Methodism in terms of distinctive doctrine. "We do not lay the main stress of our religion on any opinions, right or wrong. . . . The weight of all religion, we apprehend, rests on holiness of heart and life." It became almost an article of faith for Wesley that the believer's assent to articles of faith did not matter. The unimportance of orthodoxy was one of the first items he touched upon in his exposition of Methodism in the *Plain Account* (1748): "orthodoxy, or right opinions, is at best but a very slender part of religion, if it can be allowed to be any part of it at

² Lecky, *A History of England in the Eighteenth Century*, 3 (London, 1905): 31, 37, 137, 140; and Tyerman, *The Life and Times of the Rev. John Wesley, M.A., Founder of the Methodists*, 1 (New York, 1872): v.

³ Wesley, *The Works of John Wesley* [hereafter, *Works*], ed. Thomas Jackson, 14 vols. (1872; reprint edn., Grand Rapids, Mich., 1979), 5: 2, 3, and 7: 203, and *The Journal of the Rev. John Wesley, A.M.*, . . . [hereafter, *Journal*], ed. Nehemia Curnock, 8 vols. (London, 1909–16), 5: 117.

⁴ Wesley, *Journal*, 5: 169, and *Works*, 7: 203.

⁵ Wesley, *The Letters of the Rev. John Wesley, A.M., Sometime Fellow of Lincoln College, Oxford* [hereafter, *Letters*], ed. John Telford, 8 vols. (London, 1931), 3: 332, and 8: 89.

all.”⁶ His dismissal of orthodoxy was comprehensive and included doctrines whose truth he himself never doubted. Justification by faith, for example, was a doctrine he believed and constantly taught. Yet he never supposed that assent to this doctrine was essential to Christianity. He taught that one could be ignorant of the doctrine and yet be saved. Indeed, he believed that one could consciously reject it without danger to salvation. “Men may differ from us in their opinions, as well as their expressions, and nevertheless be partakers of the same precious faith. It is possible they may not have a distinct apprehension of the very blessing which they enjoy.”⁷

Wesley disliked the Roman Catholic Church and thought that it taught false doctrines. But he never supposed that the devout Catholic had to give up his errors in order to believe. “What a multitude of wrong opinions are embraced by all the members of the Church of Rome! Yet how highly favored have many of them been!” In this sense, it is perfectly true that Methodism makes no appeal to the believer’s understanding. It does not require assent to any particular truth. “I want you,” Wesley wrote to a friend, “not to be a convert to my opinions, but to be a member of Christ, and a child of God. . . . Be anything as to outward profession, so you are lowly in heart. . . . Be what you please besides; only be meek and gentle. . . . Hear what preacher you will; but hear the voice of God.” On Wesley’s account, the Christian could understand and accept everything that orthodoxy required and still fall short of Christian faith. “I believe the merciful God regards the lives and tempers of men more than their ideas,” Wesley wrote at the end of his life. “I believe he respects the goodness of the heart, rather than the clearness of the head.” In the end, no man could forfeit salvation “because his ideas are not clear, or because his conceptions are confused.”⁸ As a faith Methodism does not excommunicate the ignorant and the stupid. But it does not follow that the author of Methodism was necessarily ignorant and stupid or that what he said can be dismissed as emotional gibberish.

THE MATTER THAT PREOCCUPIED WESLEY was not what but how the Christian ought to believe. The great controversies of his life all turned on points of psychology. Against the Anglicans, he argued that belief was something more than reasonable assent; against the Moravians, he argued it was something that could exist in different degrees; and, against the Calvinists, it was something the Christian could choose or resist.

The psychological manifestations of belief formed the crucial issue in his own evangelical conversion in 1738. There had never been any doubt in Wesley’s mind about the means of salvation: man was saved by faith—“not having my own righteousness, but that which is through the faith of Christ, the righteousness which is of God by faith.” But did he possess this saving faith himself? “I want that faith

⁶ Wesley, *The Appeals to Men of Reason and Religion and Certain Related Open Letters* [hereafter, *Appeals and Related Letters*], ed. Gerald R. Cragg, in Frank Baker, gen. ed., *The Works of John Wesley*, 11 (Oxford, 1975): 320, *Letters*, 2: 293, *Journal*, 7: 389, and *Works*, 5: 78, and 8: 3.

⁷ Wesley, *Works*, 5: 238, and *Journal*, 5: 244.

⁸ Wesley, *Journal*, 5: 249, *Letters*, 4: 51–52, and *Works*, 7: 354.

which none can have without knowing that he hath it (though many imagine they have it, who have it not)."⁹ His lack of faith was demonstrated to him by his own state of mind. "By the most infallible of proofs, inward feeling, I am convinced . . . of unbelief." Supposing that belief was in part evidenced by love of God, Wesley wrote, "I feel this moment I do not love God; which therefore I *know* because I *feel* it. There is no word more proper, more clear, or more strong."¹⁰ After his conversion in 1738, he again appealed to feeling as proof of his belief: "I felt my heart strangely warmed. I felt I did trust in Christ, Christ alone for salvation." Wesley's conviction that belief was a sensible experience inspired by the Holy Ghost supplied the Methodist movement with its battle cry. In his controversy with Thomas Secker, Wesley identified sensible inspiration as "the main doctrine of the Methodists." Sensible inspiration was "the substance of what we all preach" and the doctrine he undertook to defend against Secker's objections. "I know the proposition I have to prove, and I will not move an hair's breadth from it. It is this: 'No man can be a true Christian without such an inspiration of the Holy Ghost as fills his heart with peace and joy and love, which he who perceives not has it not.' This is the point for which alone I contend; and this I take to be the very foundation of Christianity."¹¹

Wesley is easier to understand if we first establish what he denied sensible experience to be. He was not, indeed, talking about bodily fits and convulsions. Physical manifestations might accompany a conversion, but Wesley attached little importance to them. "I neither forward nor hinder them."¹² Such signs could be the natural reactions attending an authentic conversion; they could be the impositions of a pretended conversion.¹³ But whether false or genuine, they had nothing to do with the experience that evidenced faith. "They sometimes occur still, but not often," Wesley wrote in 1768. "And we do not regard whether they occur or not, knowing that the essence of religion . . . is quite independent upon them."¹⁴

Neither did Wesley ever suppose that the experience took the form of visions and voices in the believer's mind. He had little patience with the convert who made the mistake of experiencing too much. A number of London Methodists were expelled from the society for claiming that they were bathed in the blood of Christ. "I plainly told them," wrote Wesley, "the utmost I could allow . . . was, that some of these circumstances might be from God (though I could not affirm they were) working in an unusual manner, no way essential either to justification or sanctification; but that all the rest I must believe to be the mere empty dreams of an heated imagination."¹⁵ Whatever the believer felt, the cause of the experience and the manner in which it was inspired were not things that Wesley pretended to understand. "Such knowledge is too wonderful and excellent for me: I cannot attain unto it. The wind bloweth, and I hear the sound thereof; but I cannot tell how it cometh, or whither it goeth."¹⁶ Nor did the Methodist experience constitute some doctrinal illumination. Whatever the believer felt, it added nothing to his knowledge of Christian truth.

⁹ Wesley, *Journal*, 1: 424.

¹⁰ *Ibid.*, 415, and 2: 125–26.

¹¹ *Ibid.*, 1: 476, and *Letters*, 2: 64.

¹² Wesley, *Letters*, 2: 25.

¹³ *Ibid.*, 24, and 5: 366–67, and *Appeals and Related Letters*, 197–98.

¹⁴ Wesley, *Letters*, 5: 366.

¹⁵ Wesley, *Journal*, 3: 44, and *Letters*, 5: 365.

¹⁶ Wesley, *Works*, 5: 117, *Appeals and Related Letters*, 108, 140, and *Letters*, 2: 74, and 5: 364–65.

The inward feeling had to be tested by the authority of the written word, not the reverse.¹⁷ Wesley condemned the Quaker doctrine of the Inner Light because it usurped the teaching authority of scripture. "For though the Spirit is our principal leader, yet He is not our rule at all; the Scriptures are the rule whereby He leads us into all truth."¹⁸

And the Methodist experience cannot be equated with the Calvinist assurance, although the two are similar in many respects. The Methodist believer received no guarantee of final perseverance. Wesley rejected predestination and believed that no one was ever given an assurance of ultimate salvation. "I find it not in the Book of God. Yea, I take it to be utterly contrary thereto, as implying the impossibility of falling from grace; from asserting which fatal doctrine I trust the God whom I serve will always deliver me."¹⁹ The Methodist saint could always lapse, and any assurance he received referred to his present, not to his future, state.

The Methodist experience consisted, in part, in the emotional reactions or effects that were supposed to manifest the believer's faith. Using biblical terms, Wesley called these manifestations the fruits of faith, the fruits of the Spirit, or the testimony of the believer's spirit.²⁰ The emotions that a believer felt included peace, joy, and love; sometimes meekness, gentleness, and righteousness were added to the list.²¹ "These . . . inward fruits of the Spirit . . . must be *felt* wheresoever they are; and without these, I cannot learn from Holy Writ that any man is 'born of the Spirit.'" The experience of these effects offered the believer an indispensable and infallible evidence of his faith. In defense of Methodist conversions, Wesley once asked a doubting Moravian, "What excuse have you for not being convinced that *they* have *faith*, who have fruits which nothing but faith can produce?"²² Such manifestations were necessarily perceptible. "It cannot be in the nature of things, that a man should be filled with this peace and joy and love . . . without perceiving it as clearly as he does the light of the sun." Love and joy and peace "are inwardly felt, or they have no being." Wesley's insistence on the perceptibility of the fruits of faith predated his own evangelical conversion. In 1725 he wrote his mother, "Surely these graces are not of so little force, as that we can't perceive whether we have them or no: and if we dwell in Christ . . . certainly we must be sensible of it."²³

The fruits of faith for Wesley were the supernatural gifts of the Holy Ghost. Yet he did not imagine that their perception by the believer presupposed his possession of supernatural faculties. The believer in this respect experienced only the effects of inspiration, not its cause. The Christian felt peace, love, and joy but not the influence of the Holy Ghost itself. That such effects were divinely inspired the believer could learn only from scripture. "Whoever has these, inwardly feels them; and if he understands his Bible, he discerns from whence they come. Observe, what

¹⁷ Wesley, *Journal*, 2: 226, 381, 3: 44, and 6: 230–31, *Appeals and Related Letters*, 337, 414–15, 497, and *Letters*, 4: 33.

¹⁸ Wesley, *Letters*, 2: 117, and 4: 123–24.

¹⁹ *Ibid.*, 1: 255.

²⁰ Wesley, *Journal*, 1: 465, 471, 2: 251, and 6: 18, *Letters*, 1: 245, 329–30, 3: 160, 4: 35, 231, and 5: 8, 363, and *Works*, 5: 115, 117, 120.

²¹ Wesley, *Journal*, 6: 18, *Appeals and Related Letters*, 171, *Letters*, 1: 245, 329, 2: 64, 206, 4: 35, 331, and 5: 363, 364, and *Works*, 5: 78–79, and 8: 276.

²² Wesley, *Letters*, 1: 329–30, and 4: 35.

²³ *Ibid.*, 1: 20, 2: 64, and 4: 332.

he inwardly feels is these fruits themselves; whence they come he learns from the Bible." To account for the perception of the fruits Wesley presupposed nothing more than the believer's ordinary faculties of reflection and self-consciousness. The Christian knew that he loved God in the same way that he knew he loved any mortal. Indeed, he knew of his love of God in the same way that he knew whether he was hot or cold—that is, by sensation or by feeling. In "The Witness of the Spirit," Wesley addressed himself to the person who asked how he might know whether he possessed the fruits of faith:

I would ask him, . . . How does it appear to you, that you are alive, and that you are now in ease, and not in pain? Are you not immediately conscious of it? By the same immediate consciousness, you will know if your soul is alive to God. . . . By the same means you cannot but perceive if you love, rejoice, and delight in God. By the same you must be directly assured, if you love your neighbour as yourself.²⁴

This insistence on the perceptibility of the fruits of faith distinguishes Wesley's thought from that of the mystics. He never encouraged the believer to transcend his human senses and attempt to lose himself in some ineffable union with God. Wesley disliked the mystical conception of "naked faith" or any conception of faith that abstracted it from sensation.²⁵ Referring to nameless "Mystic Writers," Wesley wrote, "These are perpetually talking of 'self-emptiness, self-inanition, self-annihilation,' and the like: all very near akin to 'self-contradiction,' as a good man used to say." His dislike is evident even before his own conversion. "These deified men," he wrote in 1736, "enjoy such a contemplation as is not only above faith, but above sight, such as is entirely free from images, thoughts, and discourse. . . . They seek no clear or particular knowledge of anything; but only an obscure general knowledge, which is far better."²⁶

In opposition to the mystics, Wesley urged the believer to contemplate not the unknowable God but the knowable self. Only by examining his own mind could the believer judge the state of his soul. "I would . . . no more require you to cease from judging of your state by your frame of mind than I would require you to cease from breathing." And the only sanctification that the Methodist could achieve in this life was intelligible as a human state of mind: a degree of love, perceptible to the believer.²⁷ This had nothing to do with an angelic transformation of the soul or with an annihilation of the self in union with God. "I long to have you more and more deeply penetrated by humble, gentle, patient love," he wrote to a friend. "Believe me, you can find nothing higher than this till mortality is swallowed up in life." Referring again to the mystics, Wesley continued, "All the high-sounding or mysterious expressions used by that class of writers, either mean no more than this or they mean wrong. O beware of them! Leave them off before they are meddled with."²⁸

Wesley supposed there were two ways the believer could know of his forgiveness.

²⁴ *Ibid.*, 5: 363, *Journal*, 2: 125, and *Works*, 5: 114.

²⁵ Wesley, *Journal*, 6: 18, and *Works*, 6: 84, 90.

²⁶ Wesley, *Letters*, 1: 208, and 5: 313.

²⁷ *Ibid.*, 5: 200–01, and 7: 120, and *Works*, 11: 446.

²⁸ Wesley, *Letters*, 5: 342.

The first, an essentially logical proof, was the indirect testimony. Scripture told the believer whoever possessed the fruits of faith was a child of God; inward consciousness assured him that he possessed these fruits. Thereupon he might rationally conclude, "I am a child of God."²⁹ To this indirect witness Wesley added a direct testimony that the believer was enjoined to expect. Quite independently of the logical proof, the direct witness assured the believer of God's forgiveness.³⁰ This direct witness cannot simply be understood as an expression of the believer's self-confidence. Wesley supposed that by the very nature of the thing the assurance of pardon had to precede the believer's knowledge and confidence. "A man cannot have a childlike confidence in God till he knows that he is a child of God." Wesley supposed, furthermore, that the direct witness was for the believer a perceptible experience. Assuming on the authority of scripture that the direct testimony occurred, he argued that it occurred in perceptible form: "Am I not to perceive what is testified? . . . Then it is not testified at all. This is saying and unsaying in the same breath. Or am I not to perceive that it is testified to my spirit? Yea, but I must perceive what passes in my own soul!"³¹ In the perception of the fruits of the Spirit, Wesley had assumed nothing more than the believer's natural powers of self-consciousness. In the perception of the direct witness, however, he had to suppose the operation of a special faculty, a spiritual sense that only the believer could possess.

Wesley first discussed this special, spiritual sense in his *Earnest Appeal to Men of Reason and Religion* (1743). Here he denied that the mind possessed any innate ideas. What the mind knew had to come from the senses. In the case of spiritual truths, however, the physical senses were inadequate. For the perception of spiritual truths, spiritual senses were required. Without such senses the believer could no more perceive the direct testimony of the Spirit than a blind man could perceive color.³² The believer acquired his spiritual sense in his regeneration through the act of the Holy Ghost. Indeed, for Wesley, regeneration meant the acquisition of the spiritual sense. In the standard sermons he compared the unawakened Christian with the child in the womb. Both lack the sensitive faculties to perceive the reality that surrounds them. For both it is the awakening of the senses that distinguishes the birth. Christians who lack spiritual senses cannot be expected to know themselves what it is the believer experiences. And Wesley did not claim that the experience of the Christian constituted a proof for the non-Christian.³³ To explain the direct witness to the nonbeliever was like explaining sight to the blind or sound to the deaf:

He who hath that witness in himself, cannot explain it to one who hath it not: Nor indeed is it to be expected that he should. Were there any natural medium to prove, or natural method to explain, the things of God to unexperienced men, then the natural man might discern and

²⁹ Wesley, *Works*, 5: 125.

³⁰ *Ibid.*, 5: 115, 128–29, *Letters*, 5: 8, 21–22, and *Appeals and Related Letters*, 68.

³¹ Wesley, *Works*, 6: 47, and *Letters*, 2: 92, and 4: 176.

³² Wesley, *Appeals and Related Letters*, 56–57.

³³ Wesley, *Works*, 5: 224–27, and *Letters*, 2: 386.

know the things of the Spirit of God. But this is utterly contrary to the assertion of the Apostle, that "he cannot know them, because they are spiritually discerned"; even by spiritual senses, which the natural man hath not.³⁴

In the end, the spiritual sense of the believer constituted the essence of his faith. The spiritual sense "is with regard to the spiritual world," Wesley wrote in the *Earnest Appeal*, "what sense is with regard to the natural. It is the spiritual sensation of every soul that is born of God." To understand Wesley's meaning, we must recognize that this is a fundamental definition and no mere figure of speech. In 1756, he made the same point more explicitly: "Faith implies both the perceptive faculty itself and the act of perceiving God and the things of God." He resisted any attempt to identify faith with truth independently of its perception. In response to the argument that faith was what the believer believed, he objected tersely, "No it is not; no more than the light which a man sees is his sight."³⁵

In trying to define just what the believer perceived, Wesley ran into difficulties that he never satisfactorily resolved. Initially, he supposed that the believer perceived an assurance of his pardon. Without this, the believer lacked faith. "Supposing him not to see the mercy of God, then he is not a believer: For faith implies light; the light of God shining upon the soul. So far, therefore, any one loses this light, he, for the time, loses his faith." But to make consciousness of pardon a condition of pardon involved Wesley in a logical contradiction. In 1747, he wrote to his brother, "The assertion that justifying faith is a sense of pardon is contrary to reason; it is flatly absurd. . . . We have been blessed in preaching the great truths of the gospel; although we tacked to them, in the simplicity of our hearts, a proposition which was not true."³⁶ Wesley recognized the inconsistency and was troubled by it. The obvious solution was to give up the direct witness altogether and rely instead on the implied assurance that came from the fruits of the Spirit. Wesley certainly considered this possibility but rejected it in the end. He believed that the direct testimony was something Christians experienced despite the logical problems in understanding it. Moreover, he feared that a reliance on the fruits of the Spirit threatened justification by faith.³⁷

Until the end of his life, Wesley continued to insist upon the direct witness as a requirement for Christian belief. "I know not how any one can be a Christian believer till he 'hath' as St. John speaks, 'the witness in himself.'" He allowed for the possibility of exceptions. Yet these exceptions were never very convincingly accounted for. In effect, Wesley had to argue that the believer who mourned experienced something that he did not recognize.³⁸ But Wesley continued to insist upon the general rule, not the exceptions. If the Christian believed and was saved, he had to know this for himself. There was no way that anyone could tell the grieving Christian that his anxiety was unjustified. "If a man does not know the

³⁴ Wesley, *Works*, 5: 121–22.

³⁵ Wesley, *Appeals and Related Letters*, 43, and *Letters*, 3: 174, 235.

³⁶ Wesley, *Letters*, 2: 109, 3: 138, 162, and 5: 359, and *Works*, 5: 90.

³⁷ Wesley, *Letters*, 5: 8, 21–22, and *Works*, 5: 128–30.

³⁸ Wesley, *Works*, 6: 205, and *Letters*, 5: 103, 358, and 7: 298.

pardoning love of God, for himself,” Wesley asked one of his preachers, “How or by what means are you to know it for him? Has God given you to search the hearts and the reins of your hearers? Can you infallibly know the real state of that man’s mind?”³⁹

WHEN WESLEY INTERPRETED FAITH AS A SENSIBLE EXPERIENCE, he claimed no credit for originality. Indeed, he asserted the contrary. His strategy was, understandably enough, to claim that he was traditional and orthodox and that his opponents were not. On different occasions he recognized the priority of different authorities—Puritan divines like Richard Sibbes, William Perkins, and John Preston and Continental reformers like Martin Luther and Philipp Melancthon. In his controversy with William Warburton over his theory of faith, Wesley appealed to the Homilies of the Church of England.⁴⁰ The Homilies provided a source of authority that no bishop could easily repudiate. One source, however, Wesley never cited, or at least never cited with approval: the philosophy of David Hume. When Wesley thought of Hume at all, it was of Hume the infidel and skeptic—“the most insolent despiser of truth and virtue that ever appeared in the world” and “an avowed enemy to God and man, and to all that is sacred and valuable upon earth.”⁴¹

There is, nevertheless, a strong resemblance between Hume’s and Wesley’s theories of belief. In some respects, Wesley’s account of faith looks like a special application of Hume’s theory of belief in general. Neither regarded belief as the assent of the understanding. For both, it was a matter of feeling. “Belief,” wrote Hume, “is more properly an act of the sensitive than of the cogitative part of our natures.”⁴² In neither case was belief something that the human mind could create at will. For Wesley, the mind could believe only in collaboration with the Holy Ghost. “Is it,” he asked, “in your power to burst the veil that is on your heart and let in the light of eternity? You know it is not. You not only do not but cannot (by your own strength) thus believe. The more you labour so to do, the more you will be convinced, ‘it is the gift of God.’”⁴³ For Hume as well, the origins of belief were external to the will. “Belief consists . . . in something, that depends not on the will, but must arise from certain determinate causes and principles, of which we are not masters.”⁴⁴ Although in Wesley’s case the external force was God and in Hume’s it was nature, Hume’s empiricism prohibited the exclusion of any conceivable source for the sensations that the mind felt. Even without the aid of scripture, Hume had to admit that sensations could be inspired by God.⁴⁵

Both Hume and Wesley faced the same problem when they attempted to identify what it was that manifested belief to the mind of the believer. Neither could in the

³⁹ Wesley, *Letters*, 5: 104.

⁴⁰ Wesley, *Journal*, 4: 425, *Letters*, 3: 159, and *Appeals and Related Letters*, 524.

⁴¹ Wesley, *Journal*, 5: 458.

⁴² Hume, *A Treatise of Human Nature: Being an Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects*, in Thomas Hill Green and Thomas Hodge Grose, eds., *The Philosophical Works of David Hume*, 4 vols. (1886; reprint edn., Aalen, 1964), 1: 475.

⁴³ Wesley, *Appeals and Related Letters*, 48, and *Letters*, 2: 71.

⁴⁴ Hume, *Treatise of Human Nature*, 1: 555, and *An Enquiry concerning Human Understanding*, in Green and Grose, *The Philosophical Works*, 4: 41.

⁴⁵ Hume, *Treatise of Human Nature*, 1: 385.

end specify the characteristics of the sensation that they had posited. In his sermon "The Witness of the Spirit," Wesley shunned any philosophical discussion of the point. "He who hath that witness in himself, cannot explain it to one who hath it not: Nor indeed is it to be expected that he should." To know what the direct witness was required the proper spiritual senses.⁴⁶ Hume had no more success in identifying belief. "I confess," he admitted in *A Treatise of Human Nature*, "that 'tis impossible to explain perfectly this feeling or manner of conception." Belief was something that lay beyond definition. "Were we to attempt a definition of this sentiment," Hume wrote in *An Enquiry concerning Human Understanding*, "we should, perhaps, find it a very difficult, if not an impossible task; in the same manner as if we should endeavour to define the feeling of cold or passion of anger, to a creature who never had any experience of these sentiments."⁴⁷ For both Hume and Wesley, then, only a believer could tell what another believer felt.

The two theories are not, of course, identical. First, Hume's argument is that the mind feels *that* it believes; Wesley's is that the mind feels *what* it believes. Of the two, Wesley's is the less tidy and elegant. It forced him to postulate a sensation to correspond with everything the mind believes. Hume's theory did not. In other words, Hume could account for the faith of a man who believes a lie; Wesley could not. Second, Wesley's argument accounts only for the saving faith of a Christian. In contrast to Hume, Wesley said nothing about belief in general. Although in some way Wesley might have been influenced by Hume, the likelihood is very small. The similarities in their theories owe more to a common intellectual inheritance than to the chance of any direct connection between them. Both were indebted to John Locke and to the philosophical empiricism that dominated eighteenth-century thought.

The importance of Wesley's commitment to Lockean empiricism is something that is often underrated.⁴⁸ Yet there can be no doubt that Wesley knew of Locke and in general approved of his philosophy. As an undergraduate, Wesley had read Locke's *Essay concerning Human Understanding*, and as a fellow of Lincoln he on one occasion took up an abridgment of it with his students. In later life, he repeatedly recommended the *Essay* as suitable reading to devout Methodists, and in the 1780s he had lengthy extracts republished in *The Arminian Magazine*.⁴⁹ Introducing these extracts, Wesley wrote,

From a careful consideration of this whole work, I conclude that, together with several mistakes (but none of them of any great importance) it contains many excellent truths, proposed in a clear and strong manner, by a great master both of reasoning and language. It might, therefore, be of admirable use to young students, if read with a judicious Tutor, who could confirm and enlarge upon what is right, and guard them against what is wrong in it.⁵⁰

⁴⁶ Wesley, *Works*, 5: 121–22.

⁴⁷ Hume, *Treatise of Human Nature*, 1: 398, and *Enquiry concerning Human Understanding*, 4: 41.

⁴⁸ For exceptions to this generalization, see Herbert B. Workman, "The Place of Methodism in the Life and Thought of the Christian Church," in W. J. Townsend et al., eds., *A New History of Methodism*, 1 (London, 1909): 7, 16–19; and Cragg, Introduction to Wesley, *Appeals and Related Letters*, 57.

⁴⁹ V. H. H. Green, *The Young Mr. Wesley: A Study of John Wesley and Oxford* (London, 1961), 191, 315; Wesley, *Letters*, 4: 249, and 7: 82, 228; and John Wesley, ed., *The Arminian Magazine for the Year 1782, Consisting of Extracts and Original Treatises on Universal Redemption*, 5 (London, 1782) [hereafter cited by short title, volume, and year]: 27–648, *ibid.*, 6 (1783): 30–652, and *ibid.*, 7 (1784): 32–302.

⁵⁰ Wesley, *Works*, 13: 464.

Bernard Semmel has supposed that Wesley's respect for Locke sprang from a recognition of Locke's commitment to Christianity.⁵¹ No doubt, there is a side to Wesley's thought that exaggerated the philosophical importance of Christianity. Had he supposed that Locke was a bad Christian, he would probably have condemned him as a bad philosopher. But Wesley's regard for Locke rests on more substantial considerations. He was, in fact, a thoroughgoing adherent of the principles of Locke's epistemology. In the *Earnest Appeal*, Wesley denied the existence of innate ideas. What the mind could know was restricted to the ideas that it received through the senses.⁵² He identified himself again with empiricism in one of his late sermons: "For many ages it has been allowed by sensible men, *Nihil est in intellectu quod non fuit prius in sensu*." Nor was there any doubt in Wesley's mind of Locke's importance in the development of empiricist thought. Referring to Locke's *Essay*, Wesley wrote in *The Arminian Magazine*, "I think that point, 'that we have no innate principles,' is abundantly proved, and cleared from all objections that have any shadow of strength. And it was highly needful to prove the point at large . . . as it was at the time an utter paradox both in the philosophical and religious world." Moreover, he defended Locke against critics like John Norris and Thomas Reid, who objected to Locke's supposition that whatever the mind saw was seen through the medium of ideas. "Why," Wesley asked, "should any one be angry at his using the word 'idea' for 'whatever is the object of the mind in thinking'?" For Wesley, as for Locke, the idea was the basic and indispensable postulate for the explanation of human thought. "To talk of 'thinking without ideas' is stark nonsense. Whatever is presented to your mind is an idea; so that to be without ideas is not to think at all. Seeing, feeling, joy, grief, pleasure, pain are ideas. Therefore to be without ideas is to be without either sense or reason."⁵³

Wesley's concurrence with Locke was not restricted to questions of formal epistemology. Both held similar views on free will, for example. Wesley, unlike Locke, drew on scripture to make his case, but, insofar as Wesley thought of free will as a problem in philosophy, his conclusions were similar to Locke's. Like Locke, he believed that the supposition of free will is essential to any theory of moral obligation. "If man were not free," wrote Wesley, "he could not be accountable either for his thoughts, words, or actions. If he were not free . . . , he would be incapable either of virtue or of vice, of being either morally good or bad."⁵⁴ Like Locke, Wesley thought that the power to choose was something that could be empirically discovered within ourselves. "I am conscious to myself of . . . [a] property, commonly called liberty. . . . It is a power of self-determination. . . . To deny this would be to deny the constant experience of all human kind."⁵⁵ When Wesley came to quarrel with the Calvinists over predestination, he naturally turned to Locke for support. Locke's case for free will formed the largest section of his work that Wesley republished in *The Arminian Magazine*.⁵⁶

⁵¹ Semmel, *The Methodist Revolution* (New York, 1973), 87.

⁵² Wesley, *Appeals and Related Letters*, 56–57. And see page 18, above.

⁵³ Wesley, *Works*, 7: 231, and 13: 454–55, and *Letters*, 6: 229.

⁵⁴ Wesley, *Works*, 6: 227; and Locke, *An Essay concerning Human Understanding*, ed. John W. Yolton, 2 vols. (London, 1961), 1: 36.

⁵⁵ Wesley, *Works*, 7: 228; and Locke, *Essay concerning Human Understanding*, 1: 195–98.

⁵⁶ Wesley, *Arminian Magazine*, 5 (1782): 413–17, 476–78, 528–34, 585–87, 646–48, and *ibid.*, 6 (1783): 30–31, 87–89.

Wesley agreed with Locke, furthermore, in his opposition to any deductive system of theology. In *An Essay concerning Human Understanding*, Locke had denied that any science could be erected upon logical maxims alone. What was true for secular knowledge was true for divinity. "I think," Locke wrote, "that nobody will . . . say that the *Christian* religion is built on these *maxims*. . . . It is from revelation we have received it, and without revelation these *maxims* had never been able to help us to it."⁵⁷ For Wesley also, no speculative knowledge of God was possible. "'The invisible things of God,' if they are known at all, 'are known from the things that are made'; not from what God hath written in our hearts, but from what he hath written in all his works."⁵⁸

Wesley's attacks on speculative divinity sometimes read like the statements of a fundamentalist, relying solely on the authority of the written word. This is certainly true of his sermon "The Circumcision of the Heart."⁵⁹ Yet such attacks can also be understood as the objections of an empiricist against the claims of abstract reason. The empiricist content in Wesley's fundamentalism is revealed in his criticism of Andrew Ramsay and Samuel Clarke. Referring to Ramsay's *Philosophical Principles* (1749), Wesley commented, "The treatise . . . gave me a stronger conviction than ever I had before of the fallaciousness and unsatisfactoriness of the mathematical method of reasoning on religious subjects." And, he continued, "All this is beginning at the wrong end; . . . we can have no idea of God, nor any sufficient proof of His very being, but from the creatures; . . . the meanest plant is a far stronger proof hereof than all Dr. Clarke's or the Chevalier's demonstrations."⁶⁰

A distrust of reason is one of the persistent characteristics of Wesley's thought. Knowledge for him consisted not in what the mind understood but in what it experienced. Because a fact was unintelligible, it did not cease to be a knowable fact. "What pretence have I," he asked, "to deny well-attested facts, because I cannot comprehend them?" He distinguished between the intelligibility of an idea and its truth. "Those who will not believe anything but what they can comprehend, must not believe that there is a sun in the firmament. . . . They must not believe that they have a soul; no, nor that they have a body." The distinction that Wesley made between what we experience and what we understand was not restricted in its implications merely to religion. It is also crucial to the attack he made on academic medicine in his *Primitive Physic* (1747). This is the work of an extreme empiricist. He supposed that early medicine had proceeded by trial and error. Once medicine became an organized profession, however, that method gave way to speculative theorizing. "Men of learning," he wrote, "began to set experience aside, to build physic upon hypothesis, to form theories of diseases and their cure, and to substitute these in the place of experiments." In publishing *Primitive Physic*, Wesley numbered himself among the benefactors of mankind who championed experiment against speculation.⁶¹

Wesley's empiricism accounts for much of his belief in supernatural apparitions. In the case of ghosts as in the case of murder, the testimony of competent witnesses

⁵⁷ Locke, *Essay concerning Human Understanding*, 2: 199, 217–18.

⁵⁸ Wesley, *Works*, 6: 339.

⁵⁹ *Ibid.*, 5: 209.

⁶⁰ Wesley, *Letters*, 3: 104–05.

⁶¹ Wesley, *Journal*, 5: 265, and *Works*, 6: 204, and 14: 310–11.

convinced him of the reality of what he believed. "The testimony of unexceptionable witnesses fully convinces me both of the one and the other." Yet, if he was prepared to accept the existence of ghosts on empirical grounds, he refused to accept any purely speculative account of the nature of ghosts. This was his complaint against Joseph Glanvill's *Sadducismus Triumphatus* (1681): "All his talk of 'aerial and astral spirits' I take to be stark nonsense. Indeed, supposing the facts to be true, I wonder a man of sense should attempt to account for them at all. For who can explain the things of the invisible world but the inhabitants of it?"⁶² Wesley's skepticism did not, however, stop here. In the end, the real nature of visible things was for Wesley no more accessible to human reason than the real nature of Glanvill's spirits. When Wesley published his textbook on natural philosophy, *A Survey of the Wisdom of God* (1763), he disclaimed all knowledge of things in their real natures:

I endeavour throughout not to account for things, but only to describe them. I undertake barely to set down what appears in nature; not the cause of those appearances. The facts lie within the reach of our senses and understanding; the causes are more remote. That things are so, we know with certainty; but why they are so, we know not. In many cases we cannot know; and the more we inquire, the more we are perplexed and entangled.⁶³

In the visible world, the best that human reason could do was to describe and classify the information that the senses provided. The metaphysical nature of things lay beyond the reach of human understanding.

The importance of Locke's contribution to the advancement of human knowledge is incontestable. Yet we often forget that his originality lies in showing not how much but how little the mind can really understand. His epistemology ultimately restricts the limits of our knowledge and extends the areas of existence that must be abandoned to ignorance and mystery. "The simple *ideas* we receive from sensation and reflection," Locke wrote, "are the boundaries of our thoughts; beyond which the mind, whatever efforts it would make, is not able to advance one jot."⁶⁴ But neither for Locke nor for Wesley did the limits of human knowledge prescribe the limits of existence and truth. Locke posited a boundless and incomprehensible reality that existed beyond the intelligence of man. In relation to what higher beings might perceive and know of this reality, men were like worms shut up in the drawer of a cabinet located in the room of a large mansion. Just as the worm cannot know what a man perceives, so a man cannot know what higher beings might perceive. "The ignorance and darkness that is in us no more hinders nor confines the knowledge that is in others, than the blindness of a mole is an argument against the quick-sightedness of an eagle."⁶⁵

Yet Locke was convinced, no less than Wesley, that the higher reality existed and was perceived by higher beings than man. Considering the perfections of God, Locke could not bring himself to think existence was "all laid out upon so inconsiderable, mean, and impotent creature" as man. For Locke as for Wesley,

⁶² Wesley, *Journal*, 5: 266, 311.

⁶³ Wesley, *Works*, 14: 301.

⁶⁴ Locke, *Essay concerning Human Understanding*, 1: 260.

⁶⁵ *Ibid.*, 91, and 2: 159.

however, human reason was almost as impotent in the interpretation of the visible world as it was in the perception of the invisible. Like Wesley, Locke denied that man can acquire a metaphysical knowledge of the things that he perceives. The true nature of things lies beyond human understanding:

We are not capable of a philosophical *knowledge* of the bodies that are about us and make a part of us. . . . Several effects come every day within the notice of our senses, of which we have so far *sensitive knowledge*; but the causes, manner, and certainty of their production . . . we must be content to be ignorant of. . . . As to a perfect *science* of natural bodies . . . we are, I think, so far from being capable of any such thing that I conclude it lost labour to seek after it.⁶⁶

Wesley's empiricism derived ultimately from Locke. But it was not Locke so much as Peter Browne who worked the more direct and immediate influence on Wesley's mind. In Wesley's day, Browne's *Procedure, Extent, and Limits of Human Understanding* figured in the deist controversy as one of the main defenses of revelation. What distinguishes Browne's argument is the extreme empiricism of its premises. Although a Lockean in most respects himself, Browne denied Locke's contention that the mind could possess an empirical knowledge of itself. For Browne there were no ideas of reflection; all of the ideas that the mind could possess were those of external sensation. Indeed, the fact and the manner of the mind's existence could only be known through the perception of things external to itself.⁶⁷ It was a case here not of "to be is to be perceived" but of "to perceive is to be." Browne may also have anticipated Hume in denying that the mind could possess any *a priori* knowledge of causation.⁶⁸ *Limits of Human Understanding* was published in 1728, shortly before Wesley took up residence at Lincoln. The work made an immediate and lasting impression on his mind. At Lincoln, he wrote out a lengthy abstract of the work, which he showed to his friends. This he later published as an appendix to *A Survey of the Wisdom of God*, and he used Browne's treatise in the education of Methodist preachers and recommended it to Methodists for their private reading.⁶⁹

Browne's *Limits of Human Understanding* had been explicitly written for the refutation of deism.⁷⁰ Locke's *Essay concerning Human Understanding* had not. For this reason, perhaps, Wesley thought more of Browne than of Locke. Browne's work was, Wesley said, "in most points far clearer and more judicious than Mr. Locke's as well as designed to advance a better cause." And Browne, probably as much as Locke, influenced Wesley in his dislike of *a priori* theology. On epistemological grounds, Browne argued that the facts of revelation prescribed the limits of Christian belief and understanding. The revealed facts bore an analogical relationship to the realities of God, but what these realities truly were, apart from man's comprehension of the revealed analogy, formed no part of the faith that the

⁶⁶ *Ibid.*, 2: 164.

⁶⁷ Browne, *The Procedure, Extent, and Limits of Human Understanding* (1728; reprint edn., New York, 1976), 63–69, 412–14.

⁶⁸ *Ibid.*, 205–09.

⁶⁹ Wesley, *Letters*, 1: 56–58, 3: 163, and 4: 249, *Journal*, 4: 192, and Wesley, ed., *A Survey of the Wisdom of God in the Creation: or, A Compendium of Natural Philosophy*, 5 (London, 1809): 149–96.

⁷⁰ Browne, *Limits of Human Understanding*, 51.

Christian had to believe or understand.⁷¹ Here Browne taught Wesley to make a distinction between believing a Christian truth and understanding that truth. Wesley acknowledged the debt in his sermon "On the Trinity": "That great and good man, Dr. Peter Browne, . . . has proved at large that the Bible does not require you to believe any mystery at all. The Bible barely requires you to believe such facts; not the manner of them. Now the mystery does not lie in the *fact*, but altogether in the *manner*." Referring to the Trinity, Wesley asserted his belief in the fact but disclaimed all understanding of the manner. "I have no concern with it: It is no object of my faith: I believe just so much as God has revealed and no more."⁷² In the end, Wesley's confidence in the Bible derives as much from his philosophical skepticism as it does from Christian credulity.

In his conception of faith, however, Wesley owes less to Browne and Locke. Wesley regarded faith as a kind of sensitive power, but both Locke and Browne regarded it as an assent to probable truth.⁷³ Neither thought of it as a faculty, and on this point Wesley set out on his own. To be sure, he drew on a long tradition of Christian thought that distinguishes between faith and assent. He was not the first to think of faith either as a supernatural event or as a sensible experience. His idea of faith may have derived from the Quakers, the Moravians, or the Puritan divines or from scripture. What distinguishes Wesley from his predecessors here is the consistently empirical cast of his thought. Wesley's traditional, possibly enthusiastic conception of faith was sobered by a commitment to formal philosophy. The difference between Wesley and George Fox was in the end that Fox was a prophet and Wesley an intellectual.

The spiritual sense that Wesley posited is a peculiarly eighteenth-century solution to an epistemological problem. What Wesley did in divinity is similar to what Francis Hutcheson did in ethics. Both postulated a sense to account for the knowledge they supposed the mind to possess. Wesley's spiritual sense is the theological counterpart to Hutcheson's moral sense. The similarity did not, perhaps, escape Wesley's notice. Wesley disliked Hutcheson and thought that the point of his moral philosophy was to exclude God from the explanation of right and wrong. "God has nothing to do with [Hutcheson's] scheme of virtue, from the beginning to the end. So that, to say the truth, his scheme of virtue is Atheism all over. This is refinement indeed!" Yet, in its more formal and philosophically more important aspects, Wesley adhered to Hutcheson's theory. In the very passage in which he condemned Hutcheson for atheism, he accepted the existence of the moral sense. In 1780 Wesley asked in his *Thoughts upon Taste*, "Is there not a kind of internal sense, whereby we relish the happiness of our fellow-creatures, even without any reflection on our own interest, without any reference to ourselves?"⁷⁴ In the standard sermons, Wesley alluded to Hutcheson in describing the faculty that perceived the fruits of the Spirit. "Some late writers indeed have given a new

⁷¹ Wesley, *Journal*, 4: 192; and Browne, *Limits of Human Understanding*, 290–302, 473–77.

⁷² Wesley, *Works*, 6: 204.

⁷³ Locke, *Essay concerning Human Understanding*, 1: 6, and 2: 281; and Browne, *Limits of Human Understanding*, 240, 247–56. And see pages 18–19, above.

⁷⁴ Wesley, *Works*, 7: 189, and 13: 467.

name to this, and have chose to style it a *moral sense*." Wesley preferred the term *conscience*, but the difference was purely nominal. It remained "a faculty or power, implanted by God in every soul . . . , of perceiving what is right or wrong in his own heart or life."⁷⁵ In supposing the existence of a spiritual sense to account for the witness of the Spirit, Wesley did nothing in theology that he was not prepared to countenance in moral philosophy.

THE IMPORTANCE OF PHILOSOPHICAL EMPIRICISM for the interpretation of Wesley's thought was noted by Herbert B. Workman at the beginning of this century. Workman did not, however, develop his idea, and it has been ignored in subsequent scholarship. Students of Wesley's thought have tended to stress its religious, not its philosophical, background. Possibly the best-known theory of Methodism makes Wesley one of the links in a tradition of zealotry that extends through the history of the Christian church. Writing from a Roman Catholic viewpoint, Ronald A. Knox placed Wesley in a succession of enthusiasts that runs from Montanus to John of Leyden to Wesley, and on to George Baker, alias Father Divine. For Rupert E. Davies, writing from a Methodist viewpoint, that tradition represents a succession not of enthusiasts but of saints, running from Montanus to St. Francis of Assisi to Wesley, but not, of course, to Father Divine.⁷⁶ The difference between the positions of Knox and Davies is ultimately that Davies approved of the tradition and Knox did not. Over its existence and Wesley's relationship to it, however, there has been no disagreement.

But in neither version does the tradition of zealotry provide a very satisfactory explanation of what Wesley said and why he said it. In the first place, the tradition that is postulated is necessarily vague and indefinite. What identifies it in both cases is a presumed mood rather than an explicit idea; we can never be sure in the end who belongs to the tradition and who does not. If zealotry is the mark of membership, why, then, did Davies say nothing about John of Leyden and Father Divine? If it is zealotry, perhaps an imprudent zealotry, how then could Knox overlook the claims of St. Francis, Joan of Arc, or Ignatius Loyola? Yet, if Knox's purpose was only to identify heretical enthusiasts, Roman Catholics might well wonder why Wesley qualifies and Martin Luther does not. Wesley may have believed in the existence of the devil, but he never threw an ink-pot at him. The tradition that Knox and Davies have postulated is, at bottom, a journalistic fiction. It serves to group together a diverse collection of people who had little in common and nothing to distinguish them from others whom the tradition is presumably meant to exclude. To say that Wesley is an enthusiast like George Fox or a saint like Francis of Assisi blurs far too many critical distinctions to shed light on what it was that Wesley thought. Explaining Wesley in terms of this tradition, moreover, does little to account for the emergence of Methodism in a specific form and at a specific

⁷⁵ *Ibid.*, 5: 135–36.

⁷⁶ Workman, "The Place of Methodism in the Life and Thought of the Christian Church," 7, 16–17; Knox, *Enthusiasm: A Chapter in the History of Religion with Special Reference to the XVII and XVIII Centuries* (Oxford, 1950), 4, 121–22, 578; and Davies, *Methodism* (London, 1963), 11–42.

time—Methodism in its historical context. Here philosophical empiricism is better. In contrast to zealousness, philosophical empiricism is not a mood or an attitude but a definite and identifiable set of ideas. Again in contrast to zealousness, philosophical empiricism is something that enjoyed an intellectual vogue in Wesley's time. Finally, empiricism is a doctrine that we can show Wesley consciously to have espoused.

If interpretations based on enthusiasm suffer from excessive vagueness, those placing Wesley's thought in a more specific and identifiable context do not. Franz Hildebrandt and Martin Schmidt regarded Wesley primarily as a Lutheran; John Newton and Robert C. Monk have seen him primarily as a Calvinist; and Bernard Semmel has explained him primarily as an Arminian.⁷⁷ Each of these interpretations can point to an undeniable element in Wesley's thought. With the Lutherans he shared justification by faith, with the Calvinists a doctrine of assurance, and with the Arminians a doctrine of free will and sanctification. All of these accounts stress a purely theological interpretation of Wesley's thought, but the evidence that supports any one necessarily contradicts the other two. Whoever interprets Wesley's thought in theological terms must concede that it is incoherent, that Methodism is in some measure a *pastiche*. Semmel's Wesley is an Arminian. Yet Semmel had to admit that at the core of Wesley's thought is a love of the *via media* and a dislike of theological system.⁷⁸ John Newton's Wesley is a Puritan. Yet Newton was forced to admit that Wesley's Methodism is a complex entity, "a polyglot, a rope woven of various strands, a coat of many colours." Martin Schmidt's Wesley is a Lutheran. Yet Schmidt had to warn that to see Wesley "as an ideologist would be hardly to understand him at all."⁷⁹

Albert C. Outler's work perhaps represents the logical culmination of any effort to explain Wesley primarily in theological terms. In Outler's hands, Methodism becomes a complete *pastiche*. His Wesley is primarily a "folk-theologian," an "eclectic who had mastered the secret of plastic synthesis, simple profundity, the common touch." The term *pastiche* may seem inappropriate. In Wesley's eclecticism Outler claimed to discover an underlying unity. "For all his borrowing and mixing under the pressures and heat of a great popular movement," Outler asserted, "Wesley's theology emerges clear and consistent and integral." But what identifies Wesley's theological thought for Outler is merely its "sustained evangelical concern."⁸⁰ Without intending it, no doubt, Outler has yielded the debate to Wesley's critics. If nothing more than evangelical concern identifies Wesley, if no distinctive principles are clearly and coherently held, then the explanation of Methodism lies not in what Wesley thought but in what he felt.

A *pastiche* interpretation of Methodism Wesley himself rejected. "That I may say many things which have been said before, and perhaps by Calvin or Arminius, by

⁷⁷ Hildebrandt, *From Luther to Wesley* (London, 1951); Schmidt, *John Wesley: A Theological Biography*, trans. Norman P. Goldhawk, 2 vols. (Nashville, Tenn., 1972–73); Newton, *Methodism and the Puritans* (London, 1964); Monk, *John Wesley: His Puritan Heritage* (Nashville, Tenn., 1966); and Semmel, *The Methodist Revolution*.

⁷⁸ Semmel, *The Methodist Revolution*, 189–90.

⁷⁹ Newton, *Methodism and the Puritans*, 2; and Schmidt, *John Wesley: A Theological Biography*, 1: 10.

⁸⁰ Outler, ed., *John Wesley*, Library of Protestant Thought (New York, 1964), 119, 120.

Montanus or Barclay, or the Archbishop of Cambray, is highly probable. But it cannot thence be inferred that I hold 'a medley of all their principles; —Calvinism, Arminianism, Montanism, Quakerism, Quietism, all thrown together.'" Wesley insisted upon the unity and coherence of his own thought. "I believe," he wrote in 1768, "there will be found few if any real contradictions in what I have published for near thirty years." To be sure, a controversialist like Wesley is not always the most reliable judge of his own consistency. Yet to dismiss his opinion in this matter requires us to overlook what Ronald Knox has rightly called the "ultra-logical orientation" of Wesley's mind.⁸¹ Wesley was in fact a skilled logician. He had taught logic at Lincoln, he often relied on formal syllogisms in his controversial writings, and he always insisted upon the intellectual importance of logic. "What is there, then," he asked, "in the whole compass of science, to be desired in comparison of it?" His dexterity in logical debate was one of the few things in which Wesley took an un-Methodistical pride. "I have . . . found abundant reason to praise God for giving me this honest art. . . . At doing this, I bless God, I am expert; as those will find who attack me without rhyme or reason."⁸² If Methodism is to be regarded as a theological *pastische*, it cannot be accounted for by supposing that its author was a careless rhapsodist who cared nothing for logical coherence.

A theory may be coherently conceived and yet seem confused and self-contradictory when we make it answer questions it was never intended to address. In the case of Methodism, the question to be asked concerns not Wesley's theology but his epistemology. We are dealing here with a man of the eighteenth century and not the sixteenth. His intellectual outlook is formed not by the Reformation but by the Enlightenment.⁸³ In all of his controversies, he assumed the same principle: nothing is known that cannot be felt. Whether he was addressing Anglicans on the right or Calvinists and Lutherans on the left, this is the principle on which his argument stands. Against the Anglicans, he denied that we can speculatively know whether we have faith. He condemned any definition of faith that encouraged the believer to postulate his possession of it. Faith was not to be like some Cartesian idea, lurking latent and invisible in the soul of the believer. The Christian is never to be satisfied with some imperceptible state of mind. "I do not advise you to *reason* whether you have faith or not," he wrote, "but simply look up to Him that loves you for whatever you want." According to Wesley, the Christian "is not to think well of his own state till he experiences something within himself which he has not yet experienced. . . . That *something* is a living faith."⁸⁴ Here we see the Wesley who is often mistaken for an enthusiast or a ranter. Yet his argument reflects an epistemological concern that no enthusiast or ranter ever felt.

This same epistemological concern underlies his controversy with the Calvinists and Lutherans over real holiness. Wesley's opponents claimed for the believer a

⁸¹ Wesley, *Works*, 8: 374, and *Letters*, 5: 358; and Knox, *Enthusiasm*, 496.

⁸² Wesley, *Works*, 10: 483, 353.

⁸³ Semmel, *The Methodist Revolution*, 87–96. Here Semmel made an original and important argument for Wesley's membership in the Enlightenment. His Wesley, however, is an Arminian in an Enlightenment that stresses free will and not a Lockean in an Enlightenment that stresses experience.

⁸⁴ Wesley, *Letters*, 5: 151, and *Journal*, 2: 250.

righteousness that was merely imputed. Against a purely postulated state of righteousness, Wesley argued for something that was inherent and perceptible in the believer's heart. "Does a believer love God, or does he not? If he does, he has the love of God in him. Is he lowly, or meek, or patient at all? If he is, he has these tempers in himself; and if he has them not in himself, he is not lowly, or meek, or patient."⁸⁵ Wesley could conceive of no holiness that could not be felt; there was no state of mind that was imperceptible to the believer. On the surface, the Wesley who argued for inherent holiness looks like a Roman Catholic, repudiating the Wesley who had argued for faith against the Anglicans. Yet, in arguing for both faith and holiness in the believer, Wesley did not contradict himself. In both instances his point is the same: the Christian is not to rely on some postulated and imperceptible status; the only reliable guide to the Christian's state of mind is what he feels within himself.

To say that Wesley was an empiricist and that empiricism distinguishes his thought does not mean that Wesley was a bad Christian. Nor does it mean that he thought more highly of Locke than of St. Paul. No doubt, if Wesley had had to rank the saints, Paul would have come well ahead of Locke. Indeed, on one occasion he placed Locke in order of merit behind William Law and Richard Baxter.⁸⁶ Wesley cared more about religion than he did about philosophy. But it is philosophy in the end that explains what his religion meant.

⁸⁵ Wesley, *Works*, 10: 203.

⁸⁶ Wesley, *Letters*, 5: 199.

Toward a Social History of the October Revolution

RONALD GRIGOR SUNY

IN THE SIXTY-FIVE YEARS AFTER THE OCTOBER REVOLUTION, Western scholars have been unable to formulate a consensus on the reasons for the Bolsheviks' rapid rise to power between February and October 1917. Deep divisions continue to exist among academic historians in Europe and America on fundamental questions of value, causation, and methodology, while a more frigid scholarly Cold War separates Western historians from their Soviet counterparts. Carried on in the polite and moderate language appropriate to "scientific" prose, the debate over 1917 has been, despite all intentions, implicitly politicized and has involved an attempt to explain not only the progress of a major revolution but also the roots of a new type of social and political order, one that some praise as socialism and others condemn as Stalinism or totalitarianism. Because possible justifications of the "legitimacy" of the Soviet system seem to be implied in certain explanations of how the Bolsheviks came to power, much of the literature emphasizes the artificiality, the accidental or manipulated quality, of the October Revolution and concomitantly de-emphasizes the deep and long-term social developments that provided both the context and the momentum in which Lenin's party was able to emerge victorious.

Historians have understandably had difficulty separating their political preferences for or abhorrence of the Soviet Union from their treatment of the complexities of the revolutionary years. Frequently, history has been written backwards, beginning with the knowledge of the single-party dictatorship, Stalin, collectivization, and the Great Purges and retreating in time toward the heady days of 1917 to find what went wrong. Western interpretations of the Russian Revolution are arrayed all along the political spectrum, from nostalgic reactionary views regretting the passage of the tsarist regime to radical apologia for the necessity of violence and terror. But none are free, or ultimately can be free, from explicit or unconscious value judgments about the benefits or costs of this revolution.

Earlier versions of this essay were delivered as talks at Swarthmore College and to the National Seminar on the History of Russian Society in the Twentieth Century. This version owes much to critical readings by Geoff Eley and William G. Rosenberg of the University of Michigan, Alexander Rabinowitch of Indiana University, Bloomington, and Allan K. Wildman of Ohio State University. All dates are Old Style.

Although history is never entirely free of ideological preconditioning and cannot be completely "objective," historians are still obligated to be clear about their values and preferences and their politics and to attempt, within these limits, to be as sensitive and fair to the evidence as possible. Not surprisingly, most Western historians of the Russian Revolution come to the study of 1917 committed to an evolutionary, democratic political and social system and are highly suspicious of the possibility of creating a noncapitalist, socialist economic order—particularly out of whole cloth, as the Bolsheviks attempted to do. Despite frequent claims of detachment and objectivity, scholars often make their judgments about the revolution and the Soviet Union against the standard of quite different European and American experiences. It is helpful, therefore, to explore the connection between conscious and unconscious ideological intrusions and the approach to a historical problem, for a particular angle of vision may illuminate some aspects while obscuring others.

Not unconnected to political values brought to the study of the revolution are the kinds of analyses preferred by most Western historians. Up until the last decade, Western historiography of 1917 has been concerned primarily with political explanations, emphasizing the importance of governmental forms and ideas but underestimating the more fundamental social and economic structures and conflicts in Russian society. Approaching the revolution from the top down, these writers have been concerned with the politics of the tsarist and provisional governments, with parties and revolutionary organizations, and with the dynamic personalities of Lenin, Trotsky, and Kerensky as well as with ideological questions.¹ This tendency to stay with the most articulate political actors remains dominant to the present, although several recent studies of political parties incorporate sociohistorical material.²

Russian history in general and the study of the revolution in particular have been latecomers to social history. This might seem anomalous to people outside the field, given that scholars in the Soviet Union write exclusively in a Marxist tradition. But during the Stalinist years Soviet Marxist historiography of the revolution was limited to explorations of the role of the party and key party leaders, and only recently have there been more broadly gauged investigations of workers, peasants, the soviets, and other mass organizations and movements.³ In the West, the

¹ See, for example, George Katkov, *Russia 1917: The February Revolution* (New York, 1967); Rex A. Wade, *The Russian Search for Peace: February to October 1917* (Stanford, 1969); Adam B. Ulam, *The Bolsheviks: The Intellectual and Political History of the Triumph of Communism in Russia* (New York, 1965); John Shelton Curtiss, *The Russian Revolutions of 1917* (Princeton, 1957); and John M. Thompson, *Revolutionary Russia, 1917* (New York, 1981).

² See Oliver Radkey, *The Agrarian Foes of Bolshevism: Promise and Default of the Russian Socialist Revolutionaries, February to October 1917* (New York, 1958), and *The Sickle under the Hammer: The Russian Socialist Revolutionaries in the Early Months of Soviet Rule* (New York, 1963). On the Mensheviks, see Leopold H. Haimson, ed., *The Mensheviks: From the Revolution of 1917 to the Second World War* (Chicago, 1974); on the Kadets, see William G. Rosenberg, *Liberals in the Russian Revolution: The Constitutional Democratic Party, 1917–1921* (Princeton, 1974); on the Bolsheviks, see Alexander Rabinowitch, *Prelude to Revolution: The Petrograd Bolsheviks and the July 1917 Uprising* (Bloomington, Ind., 1968), and *The Bolsheviks Come to Power: The Revolution of 1917 in Petrograd* (New York, 1976); and, on the anarchists, see Paul H. Avrich, *The Russian Anarchists* (Princeton, 1967).

³ Among Soviet works dealing with the social history of the revolution, see E. N. Burdzhakov, *Vtoraia russkaia revoliutsiia*, 2 vols. (Moscow, 1967–71); A. M. Andreev, *Sovety rabochikh i soldatskhkh deputatov nakanune oktiabria* (Moscow, 1970); L. S. Gaponenko, *Rabochii klass Rossii v 1917 godu* (Moscow, 1970); G. L. Sobolev,

sociohistorical approach has also long been hindered by the inaccessibility of Soviet archives, but in the last decade the picture has changed dramatically. Scholars have turned away from the most visible participants in the revolution to look at the rest of Russian society and to outlying regions and have produced a number of sociohistorical studies—of workers (by Mark David Mandel, Robert Devlin, William G. Rosenberg, Diane Koenker, Stephen A. Smith, Ziva Galili y Garcia), of revolution in the provinces (by Donald J. Raleigh, Roger Pethybridge, Ronald Grigor Suny, Andrew Ezergailis), of the soldiers (by Allan K. Wildman), of the sailors (by Norman E. Saul, Evan Mawdsley), and the whole array of spontaneous mass organizations (by Marc Ferro, Oskar Anweiler, Rex A. Wade, John L. H. Keep, Tsuyoshi Hasegawa).⁴ The Russian peasantry, hitherto largely neglected in the revolutionary period, is now the subject of a monograph by Graeme J. Gill.⁵

The debate about social and political history has been both fruitful and divisive within the profession—fruitful in that it has forced more conscious appreciation by all historians of the tasks in which they are engaged, but divisive in that it has hardened positions and distorted the dialogue between Marxist and non-Marxist social historians.⁶ The study of the Russian Revolution has been spared the

Revoliutsionnoe soznanie rabochikh i soldat Petrograda v 1917 godu (Leningrad, 1973); V. I. Miller, *Soldatskie komitety russkoi armii v 1917 g.: Vozniknovenie i nachalnyi period deiatel'nosti* (Moscow, 1974); P. N. Pershin, *Agrarnaia revoliutsiia v Rossii*, 2 vols. (Moscow, 1966); and P. V. Volobuev, *Proletariat i burzhuaia v Rossii v 1917 godu* (Moscow, 1964). For discussions of Soviet historiography, see David A. Longley, "Some Historiographical Problems of Bolshevik Party History (The Kronstadt Bolsheviks in March 1917)," *Jahrbücher für Geschichte Osteuropas*, 22 (1975): 494–514; and John L. H. Keep, "The Great October Socialist Revolution," in Samuel H. Baron and Nancy W. Heer, eds., *Windows on the Russian Past* (Columbus, Ohio, 1977), 139–56.

⁴ On workers, see Mandel, "The Development of Revolutionary Consciousness among the Industrial Workers of Petrograd between February and November 1917" (Ph.D. dissertation, Columbia University, 1977); Devlin, "Petrograd Workers and Workers' Factory Committees in 1917: An Aspect of the Social History of the Russian Revolution" (Ph.D. dissertation, State University of New York, Binghamton, 1976); Rosenberg, "Workers' Control on the Railroads and Some Suggestions concerning Social Aspects of Labor Politics in the Russian Revolution," *Journal of Modern History*, 49 (1977): D1181–D1219, and "The Democratization of Russia's Railroads in 1917," *AHR*, 86 (1981): 983–1008; Koenker, *Moscow Workers and the 1917 Revolution* (Princeton, 1981); Smith, "The Russian Revolution and the Factories of Petrograd: February 1917 to June 1918" (Ph.D. dissertation, Birmingham University, 1980), and "Craft Consciousness, Class Consciousness: Petrograd 1917," *History Workshop*, 11 (1981): 33–56; and Galili y Garcia, "The Menshevik Revolutionary Defensists and the Workers in the Russian Revolution of 1917" (Ph.D. dissertation, Columbia University, 1979). On the provinces, see Raleigh, "The Russian Revolutions of 1917 in Saratov" (Ph.D. dissertation, Indiana University, Bloomington, 1979), and "Revolutionary Politics in Provincial Russia: The Tsaritsyn 'Republic' in 1917," *Slavic Review*, 40 (1981): 194–209; Pethybridge, *The Spread of the Russian Revolution: Essays on 1917* (London, 1972); Suny, *The Baku Commune, 1917–1918: Class and Nationality in the Russian Revolution* (Princeton, 1972); and Ezergailis, *The 1917 Revolution in Latvia* (Boulder, Colo., 1974). On the soldiers, see Wildman, *The End of the Russian Imperial Army: The Old Army and the Soldiers' Revolt (March–April 1917)* (Princeton, 1980). On the sailors, see Saul, *Sailors in Revolt: The Russian Baltic Fleet in 1917* (Lawrence, Kans., 1978); and Mawdsley, *The Russian Revolution and the Baltic Fleet: War and Politics, February 1917–April 1918* (London, 1978). And, on mass organizations, see Ferro, *The Russian Revolution of February 1917* (London, 1972), and *October 1917: A Social History of the Russian Revolution* (London, 1980); Anweiler, *The Soviets: The Russian Workers', Peasants', and Soldiers' Councils* (New York, 1974); Wade, "Spontaneity in the Formation of the Workers' Militia and Red Guards, 1917," in Ralph Carter Elwood, ed., *Reconsiderations on the Russian Revolution* (Cambridge, 1976), 20–41, and *Red Guard and Workers' Militias: Spontaneity and Leadership in the Russian Revolution* (Stanford, 1983); Keep, *The Russian Revolution: A Study in Mass Mobilization* (New York, 1976); and Hasegawa, *The February Revolution: Petrograd, 1917* (Seattle, 1980).

⁵ Gill, *Peasants and Government in the Russian Revolution* (London, 1979).

⁶ For the debate, see Gareth Stedman Jones, "From Historical Sociology to Theoretical History," *British Journal of Sociology*, 27 (1976): 295–305; Tony Judt, "A Clown in Regal Purple: Social History and the Historians," *History Workshop Journal*, 7 (1979): 66–94; Elizabeth Fox Genovese and Eugene D. Genovese, "The Political Crisis of Social History: A Marxist Perspective," *Journal of Social History*, 10 (1976): 205–20; and Louise Tilly et al., "Problems in Social History: A Symposium," *Theory and Society*, 9 (1980): 667–81.

sparring that has divided European social historians, in part because Russian scholars have neglected or consciously avoided theoretical and methodological issues and in part because they have not written much nonnarrative, quantitative, apolitical social history of the sort done by some family historians.⁷ Russian social history has been more concerned with the movement and movements of social groups and classes than with patterns of fertility or mortality and has emphasized those moments of intense conflict, such as 1905 and 1917. That focus has always demanded some attention to politics. An appreciation of the estate (*soslovie*) structure of Russian society and the emergence of classes has long been a part of this tradition of historical writing. Yet there have been influential examples of histories written without much concern with the underlying social context as well as studies that have dealt almost exclusively with social movements without adequate consideration of political issues. And, perhaps most surprisingly, the October Revolution has often been studied without taking into account the sociopolitical developments of the late tsarist period, particularly the crucial last years of constitutional impasse and rising urban unrest.

THE OVERTHROW OF THE TSAR, accomplished by workers and soldiers in Petrograd, was the product of largely spontaneous action by thousands of hungry, angry, and war-weary women and men who had lost all confidence in the government of Nicholas II. But, along with the political revolution aimed at autocracy, a deeply rooted social antagonism, particularly on the part of certain groups of workers, against the propertied classes (the so-called *tsenzovoe obshchestvo*) was evident. This social cleavage was not simply a product of the war years but antedated that conflict, as Leopold H. Haimson showed in a two-part, seminal article published more than a decade ago. Haimson argued that a dual polarization had been taking place in urban Russia in the last years before the war. As all but the most conservative strata of society moved away from the bureaucratic absolutist regime, the working class—or, more precisely, workers in large firms, such as the metallurgical plants—was pulling away from the liberal intelligentsia, from moderates in the Social Democratic party, and from Duma politicians. “By 1914,” Haimson stated, “a dangerous process of polarization appeared to be taking place in Russia’s major urban centers between an *obshchestvo* [“society”] that had now reabsorbed the vast majority of the once-alienated elements of its intelligentsia—and that had even begun to incorporate many of the “workers’ own intelligentsia”—“and a growing discontented and disaffected mass of industrial workers, now left largely exposed to the pleas of an embittered revolutionary minority.”⁸

In contrast to the usual picture of the Bolsheviks as an isolated clique among a working class generally concerned with economic issues, the workers became steadily and increasingly radicalized in the metal industry and in St. Petersburg

⁷ For a statement of this view, see Edward Shorter’s belligerent contribution to the symposium “Problems in Social History” in *Theory and Society*, 9 (1980): 670–74. For a plea for the reconciliation of the various forms of social history, see Charles Tilly’s contribution, *ibid.*, 679–81.

⁸ Haimson, “The Problem of Social Stability in Urban Russia, 1905–1917,” *Slavic Review*, 23 (1964): 639; for the entire article, see *ibid.*, 619–42, and *ibid.*, 24 (1965): 1–22.

particularly such that Bolshevik influence grew at the expense of the Mensheviks and Socialist Revolutionaries (SRs). Haimson's work of the last decade demonstrates that workers had an increasing sense of class unity and separation from the rest of society as well as an awareness that they could solve their own problems. Ever more militant and far-reaching demands were put forth, most notably by St. Petersburg metalworkers, and the high incidence of defeat in their economic strikes only propelled them further toward a revolutionary opposition to the regime and the industrialists. "Given the even more precise correspondence between the image of the state and society that the Bolsheviks advanced and the instinctive outlook of the laboring masses, the Bolshevik party cadres were now able to play a significant catalytic role. They succeeded . . . in chasing the Menshevik 'Liquidators' out of the existing open labor organizations."⁹ By 1914 the key labor unions were in the hands of the Bolsheviks, and working-class discontent exploded in a sharp increase in the number and duration of strikes and political protests.

While the war years demonstrated the fragility of the Bolsheviks' newly conquered positions within the working class and arrests and patriotism ate into their influence, the potential for a renewal of militance remained intact. Much more visible than the exiled Bolshevik leaders were those more moderate socialists who remained in the capital and worked in the legal and semilegal institutions permitted by the autocracy. With the collapse of tsarism, timing and geography promoted even the less-prominent Mensheviks and Socialist Revolutionaries into positions of enormous power and influence. Although in the first month of revolution workers were neither unified around any one program nor tightly tied to any one party, there was a striking consensus among most Petrograd workers on the question of power in both the state and the economy.¹⁰ Except for the most militant, the metalworkers of the Vyborg district, the workers were not yet anxious either to take state power or to run the factories themselves. Thus, there was a strategic parallel between their conditional support of the Provisional Government—*poskol'ko, postol'ko* ("insofar as") their policies corresponded to the interests of the Soviet—and the notion of *rabochii kontrol'* ("workers' control"), which at this time meant merely the supervision of the owners' operations by representatives of the workers, not the organization of production directly by the workers.¹¹ Both the political and economic policies favored by active workers in the first months of revolution entailed watching over and checking institutions that continued to be run by members of propertied society.

Yet the social polarization that Haimson has noted was already evident even in the euphoria of February and early March, as the workers and soldiers set up their own class organizations—factory committees, soldiers' committees, militia, and,

⁹ *Ibid.*, 23: 638.

¹⁰ For a detailed treatment of the revolutionary days of February–March, see Hasegawa, *The February Revolution: Petrograd, 1917*, 215–409; and, on the question of party consciousness outside the capital, see Diane Koenker, "The Evolution of Party Consciousness in 1917: The Case of the Moscow Workers," *Soviet Studies*, 30 (1978): 38–62.

¹¹ There has been some debate on the exact meaning and dimensions of workers' control in 1917–18. See Chris Goodey, "Factory Committees and the Dictatorship of the Proletariat (1918)," *Critique*, 3 (1974): 27–47; M. Brinton, "Factory Committees and the Dictatorship of the Proletariat," *ibid.*, 4 (1975): 78–86; and William G. Rosenberg, "Workers and Workers' Control in the Russian Revolution," *History Workshop*, 5 (1978): 89–97.

most importantly, the soviets—to articulate and defend their interests.¹² From the beginning of the revolution they registered a degree of suspicion toward the Duma Committee and the Provisional Government, even though significant concessions were made to the representatives of educated society. Among the rank-and-file soldiers the sense of distance and distrust toward their officers led them to form their own committees and draft the famous Order Number One, which both legitimized the committees and placed the Petrograd garrison under the political authority of the Soviet. Among the sailors of the Baltic Fleet, a force in which workers were much more heavily represented than in the peasant-based army, the hatred of the crewmen toward the officer elite resulted in an explosion of summary “executions.”¹³ The genuine suspicions of the *demokratiia* (“lower classes”) were reflected by their leaders, who rejected any notion of a coalition government with the bourgeoisie and maintained that the Soviet should remain a separate locus of power critical of but not actively opposing the government. Thus, *dvoevlastie* (“dual power”) was an accurate mirror of the real balance of forces in the city and the mutual suspicion that kept them from full cooperation.

The irony of the February Revolution was that the workers and soldiers had effectively overthrown the old government, but neither they nor their leaders were yet confident enough of their abilities to form their own government or to prevent a counterrevolutionary challenge if the propertied classes were excluded. At the same time that soldiers and workers were reluctant to be ruled by their old class enemies, they realized that without agreement with the Temporary Duma Committee the loyalty of the army at the front was problematic.¹⁴ The Duma leadership, for its part, understood that real power—the power to call people into the streets, defend the city, make things work or fall apart—was in the hands of the Soviet, not the government. Moderate leaders in both the government and the Soviet were willing to play down the conflict within society in the face of a possible reaction from the right. Realism and caution through March and early April allowed a brief period of cooperation and conciliation that at first convinced many of the possibility of collaboration between the top and bottom of society but that ultimately created, when collaboration failed, a bitter and divisive aftermath.

As early as March 10 the Soviet and the Petrograd Society of Industrialists agreed to introduce an eight-hour working day in the factories. This victory for the workers on an issue that had caused deep hostility in the prewar period was achieved with surprising ease, and the conciliatory attitude of industrialists like

¹² Tsuyoshi Hasegawa, “The Formation of the Militia in the February Revolution: An Aspect of the Origins of Dual Power,” *Slavic Review*, 32 (1973): 303–22, “The Problem of Power in the February Revolution of 1917 in Russia,” *Canadian Slavonic Papers*, 14 (1972): 611–32, and “The Bolsheviks and the Formation of the Petrograd Soviet in the February Revolution,” *Soviet Studies*, 39 (1977): 86–106.

¹³ Saul, *Sailors in Revolt*, 15–16. Saul denied that the executions of officers provide evidence of “class warfare” in the navy, but after eliminating other reasons for the violence he has left the reader without any convincing explanation; *ibid.*, 77–80. Mawdsley has portrayed the mutiny in February as the result of accumulated grievances—involuntary service, low pay, inactivity, harsh discipline, and the poor treatment of sailors by officers. But what made these problems so much more explosive in the navy than in the army was that sailors were recruited from the working class, were relatively well educated, and were quite young. They were highly susceptible to socialist propaganda and responded to developments among the urban workers. Mawdsley, *The Russian Revolution and the Baltic Fleet*, 2–10.

¹⁴ Wildman, *The End of the Russian Imperial Army*, 172.

Konovalov seemed to predict further concessions. Demands for higher wages were met with sympathy, and during the first three months of the revolution nominal wages rose on the average of 50 percent in Russia.¹⁵ Although there was greater resistance to the idea of a minimum wage, it too was finally approved by the industrialists on April 24. In a sense workers were trespassing on prerogatives traditionally held by capitalists when they demanded the removal of unpopular administrative personnel, but early in the revolution even such desires as these were satisfied.

As Ziva Galili y Garcia has convincingly shown, workers' expressions of suspicion toward the "bourgeoisie" declined substantially in March, but significant groups within the industrial class began to express their opposition to the "excessive demands" of the workers.¹⁶ Even Konovalov, an advocate of cooperation with the workers, held that the overthrow of tsarism should rightly result in the establishment of the commercial-industrial bourgeoisie as the dominant force in Russia's social and economic life. Although this notion seems to coincide with the Menshevik conception of the revolution as "bourgeois-democratic," serious tactical differences emerged between the Progressist leaders, representing powerful industrialists, and the Revolutionary Defensists, who led the Soviet. Whereas the First Congress of Trade and Industry called for restoration of "free trade" and the placing of food supply in the hands of the "experienced commercial-industrial class," the Menshevik economists favored price regulation and state control of the economy. But the issue that brought the fragile dual-power arrangement down was not the emerging economic issue but the conflict between the upper and lower classes over the war.

Initially, the soldiers were suspicious of dual power and even of the Soviet to some extent, but Allan K. Wildman has demonstrated that, as a result of a campaign by the "bourgeois" press to turn the army against workers struggling for the eight-hour day and of a successful propaganda effort by Soviet agitators directed at the lower ranks, soldiers began to perceive the Provisional Government as a "class" instead of a "national" institution.¹⁷ One by one the soldiers' congresses held at the various fronts came out in support of Soviet control over the government and a "democratic peace without annexations or contributions"—the positions taken by the Revolutionary Defensists. The April crisis marked the end of the futile attempt by Miliukov and his closest associates to maintain a foreign policy independent of the Soviet. The same cleavage that was visible in Petrograd between the *demokratiia* and the *tsenzovoe obshchestvo* on the questions of power, the economy, and the war was also reflected within the army between the soldiers and their officers.

The dependence of the Provisional Government on the Soviet, clear from the first days of their coexistence, required in the view of the members of the government the formation of a coalition. At first resistant to joining a government of the bourgeoisie, the Mensheviks reluctantly agreed in order to bolster the government's authority. For the Revolutionary Defensist leader Irakli Tsereteli,

¹⁵ Galili y Garcia, "The Menshevik Revolutionary Defensists and the Workers," chap. 2: 16.

¹⁶ *Ibid.*, 27.

¹⁷ Wildman, *The End of the Russian Imperial Army*, 320.

coalition meant the unification of the workers with other “vital forces of the nation” in an effort to end the war and fight social disintegration. The successful collaboration of the bourgeoisie and the Soviet in the first months of the revolution had lulled the Mensheviks into a belief that class hostility could be overcome, but almost simultaneously with the formation of coalition the economic situation grew worse. Inflation forced more demands for wage increases, but industrialists who had recently been so generous now were resistant to further raises. In May and June workers began to suspect that factory shutdowns were deliberate attempts at sabotage by the owners. Economic difficulties, so intimately tied to the war, turned workers against the industrialists and the government.¹⁸ Although some workers supported coalition, the great bulk of Petrograd’s factory workers grew increasingly suspicious both of the government and of those socialists who collaborated with the bourgeoisie. The beneficiaries of this suspicion and distrust were those parties that opposed the coalition and advocated a government composed of the representatives of the working people—that is, the Bolsheviks.¹⁹

The association of the Menshevik and SR leaders of the Soviet with the coalition government—and, consequently, with the renewed war effort in June—placed a stark choice before the workers and soldiers: either cooperation and collaboration with the upper classes, who were increasingly perceived as enemies of the revolution, or going it alone in an all-socialist soviet government. The first efforts of the *demokratiia* were directed at convincing the Soviet leaders of the necessity of taking power in their own hands. The erosion of lower-class support for the government was already quite clear on May 31 when the workers’ section of the Petrograd Soviet voted for the Bolshevik resolution calling for “All Power to the Soviets!” Even more dramatic was the demonstration of June 18, in which hundreds of thousands of workers marched carrying slogans such as “Down with the Ten Capitalist Ministers!” By early July, with the distressing news of the failure of the June offensive filtering into the city, the more militant soldiers, sailors, and workers attempted through an armed rising to force the Soviet to take power. Emblematic of the paradox of the situation is that famous scene when sailors surrounded the SR leader Chernov and yelled at him, “Take power, you son-of-a-bitch, when it is given to you.”²⁰

But, as is well known, the Soviet did not take power, and a series of weak coalition governments followed the July crisis until their forcible overthrow in October. The brief eclipse of the Bolsheviks in July and their rapid rise from isolation and persecution through the summer to state power in October have been the object of

¹⁸ Galili y Garcia, “The Menshevik Revolutionary Defensists and the Workers,” chap. 6: 3–17.

¹⁹ The predominant Western image of the Bolshevik party as a party of *intelligenty* divorced from the working class has been challenged in quantitative studies by William Chase and J. Arch Getty on the Moscow Bolsheviks. They have concluded that the party, while “primarily composed of and dominated by” *intelligenty* up to 1905, “so radically altered its social composition [after 1905] that, by 1917, the Bolsheviks could honestly claim to represent a large section of the working population”; Chase and Getty, “The Moscow Bolshevik Cadres of 1917: A Prosopographical Analysis,” *Russian History*, 5 (1978): 95, 84–105.

²⁰ Rabinowitch, *Prelude to Revolution*, 188. The source for this scene is P. N. Miliukov, *Istoriia vtoroi russkoi revoliutsii*, 1 (Sofia, 1921): 244. Leon Trotsky called Miliukov’s account “nothing more than anecdote,” but one that “expresses with crude accuracy the essence of the July situation”; Trotsky, *The History of the Russian Revolution*, trans. Max Eastman, 2 (Ann Arbor, 1957): 40.

an enormous amount of historical study, but in their search for explanations historians have tended to overemphasize the role of political actors, like Lenin and Trotsky, and to underestimate the independent activity of workers and soldiers. Before setting out a new sociopolitical interpretation of the Bolshevik victory emerging from recent and ongoing research, I shall consider the limitations exhibited by several influential works that focus too exclusively on either the political or the social aspects of the revolution.

ABOUT A DECADE AND A HALF AGO, one of the most stimulating views of the October Revolution was what might be called the “conservative-accidentalist” interpretation shared by Sergei Petrovich Melgunov and Robert V. Daniels.²¹ Melgunov, who was a minor participant in the events he described, revealed his approach in his opening quotation from Kerensky: “By the will of men, not by the force of the elements, did October become inevitable.” To Melgunov, the October Revolution was in no sense inevitable; indeed, he made clear from the beginning his disagreement with those historians for whom “social processes almost fatalistically predetermined the course of events. . . . ‘October’ was not the realization of ‘February.’”²² Melgunov deliberately distinguished his approach from his notion of the Marxist approach to history—inevitability, social determinism, and the idea that revolutions, like societies, march lock step through a series of succeeding stages.

Melgunov emphasized instead the power and persuasiveness of Lenin, who singlehandedly was able to turn the central committee of the Bolshevik party from moderation to the radical alternative of seizing power by force. Melgunov treated Lenin as if he had been mad: “With the stubbornness of a maniac under self-hypnosis, he insisted *now or never*. . . . The uprising had become an obsession with Lenin.”²³ Lenin was “still raving” on page 7, made “hysterical demands” on page 9, and fell “into a complete fit of rage” on page 16. Moreover, the government was lulled to sleep. With the exception of the energetic prime minister, Kerensky, the cabinet had, in Melgunov’s terminology, become “spineless.” No adequate measures were taken to stop the Bolsheviks until it was too late. There was a false sense of confidence that the government with the support of the garrison could put down a Bolshevik uprising.²⁴ Thus, in Melgunov’s treatment the Bolshevik seizure of power appears to have been a matter of competing wills—the determined will of Lenin to take power by any means before the Second Congress of Soviets, the misguided will of Kerensky to allow the Bolsheviks to make a move so that they could be exposed and crushed, and the vacillating wills of cabinet officers and the military who did not take resolute action in time. Once the Bolsheviks began their occupation of government buildings, the Kerensky government found no one in Petrograd willing to defend the Winter Palace, except some military cadets and the famous Women’s Battalion of Death.

²¹ Melgunov, *Kak Bol'sheviki zakhvatili vlast': Oktiabr'skii perevorot 1917 goda* (Paris, 1953), translated, abridged, and edited by Sergei G. Pushkarev as *The Bolshevik Seizure of Power* (Santa Barbara, 1972); and Daniels, *Red October: The Bolshevik Revolution of 1917* (New York, 1967).

²² Melgunov, *The Bolshevik Seizure of Power*, 2, 3.

²³ *Ibid.*, 5.

²⁴ *Ibid.*, 29–33.

The Kerensky government's inability to mobilize military and popular support in the moment of crisis is well known and indisputable, but Melgunov argued further that the Bolsheviks, too, lacked mass support in the October Days. The garrison, on the whole, remained neutral; "only individual soldiers or sub-regimental units—at best"—came out into the streets at the call of the Military Revolutionary Committee. Workers and their Red Guard units participated in the uprising only sporadically. The decisive force on the Bolshevik side were the sailors of Kronstadt and Helsinki, who arrived in Petrograd on the afternoon of October 25. Here, then, was the extent of mass backing for the Bolsheviks: a few strategically placed armed units of soldiers, sailors, and workers. "The Russian public was almost completely absent on that tragic day."²⁵

Melgunov conceded that the Bolsheviks did manage to have enough force at the right place to win the day in October, and he quoted Lenin in seeming agreement: "To have an overwhelming superiority of forces at the decisive moment at the decisive point—this law of military success is also a law of political success." With their "powerful backing in the capital cities," the Bolsheviks were able to take over the rest of the country.²⁶ Melgunov did not explain how the Bolsheviks received the backing that they managed to mobilize in October, nor did he explain why the "revolutionary democracy proved impotent," except to note that the forces in the center had no unity of will.²⁷ This, of course, begs the question why there was no unity of will among the moderates in the face of their impending loss of power. Why were the Bolsheviks capable of the necessary will and determination and not Kerensky or the moderate socialists? Indeed, the moderates had leaders of determination and will, most notably the Menshevik Tsereteli and the conservative General Kornilov. Yet their attempted political solutions failed. Ultimately, the momentous events in October require more than explanations based on such accidental qualities as personality and mood.

In his *Red October*, published to mark the fiftieth anniversary of Lenin's seizure of power, Robert Daniels portrayed the revolution as a "veritable orgy of democracy," "galloping chaos," and "violent political struggle." Although Daniels acknowledged early in his book that the workers of Petrograd "played a decisive role in the flow of events because they were the strongest social force in the deciding center of the country, the capital city,"²⁸ he did not provide much in the way of an explanation for their leftward shift; indeed, the body of his book essentially ignores the role of the workers. Daniels's view echoes that of Melgunov: the people in power were indecisive, and one party—the Bolsheviks—was able and willing to take decisive action. The October insurrection, largely the product of Lenin's determination, "succeeded against incredible odds" and was a "wild gamble, with little chance that the Bolsheviks' ill-prepared followers could prevail against all the military force that the government seemed to have. . . . To Lenin, however, it was a gamble that

²⁵ *Ibid.*, 63, 75.

²⁶ *Ibid.*, 193. Melgunov went on to say, "There is no question that capitals decide the political fate of nations to a considerable degree."

²⁷ Melgunov, *The Bolshevik Seizure of Power*, 193.

²⁸ Daniels, *Red October*, 11.

entailed little risk, because he sensed that in no other way and at no other time could he have any chance at all of coming to power.”²⁹

Daniels’s idea that Lenin produced the October crisis, that he “singlehandedly polarized Russian political life in the fall of 1917,” is crucial to what followed:

If the revolution had not occurred as it did, the basic political cleavage of Bolsheviks and anti-Bolsheviks would not have been so sharp, and it is difficult to imagine what other events might have established a similar opportunity for one-party Bolshevik rule. Given the fact of the party’s forcible seizure of power, civil violence and a militarized dictatorship of revolutionary extremism followed with remorseless logic.³⁰

But Lenin’s victory was neither inevitable nor necessitated by social causes. Lenin was personally responsible for and indispensable to the Bolshevik victory; if he had been kept out of Russia in 1917 or had been “recognized by the cadet patrol” that stopped him on the way to Smolny on the very eve of the October Revolution, “his followers could not have found a substitute.”³¹ Thus, for Daniels, October is a historical accident contingent upon just the right, somewhat arbitrary elements, present at just the right moment. There is no sense here, except for a few sentences in the first pages, of the great social and economic forces at play in 1917, the movements of workers and soldiers, that made Lenin’s success possible and Kerensky’s defeat a near certainty. Such a sense, of course, cannot be gained from a largely biographical or political approach to the revolutionary events of 1917.³²

Useful as a corrective to the approach of Melgunov and Daniels is the work of Alexander Rabinowitch, a meticulous student of the Bolshevik party in 1917, who has attacked the cliché that the key element in Bolshevik success was the party’s superior leadership and organization. This stereotype of Bolshevism stems from a reading of Lenin’s 1902 tract, *Chto delat’?* (“What Is to Be Done?”), in which Lenin put forth an image of a centralized, disciplined party of underground, professional revolutionaries. Such a party was the crucial instrument, it is often argued, that brought off the *coup d’état* of October and established order in the midst of anarchy. Rabinowitch has argued instead that this stereotype is overdrawn, that “the party’s internally relatively democratic, tolerant, and decentralized structure and method of operation, as well as its open and mass character,” made possible a flexible, dynamic relationship between the party hierarchy and its potential supporters.³³

²⁹ *Ibid.*, 215–16.

³⁰ *Ibid.*, 81, 218.

³¹ *Ibid.*, 225.

³² Another result of the focus on personalities and parties has been a sense of the mass activity of the lower classes being without form or meaning. Activity of workers and soldiers is equivalent to anarchy for many historians. This theme of the revolution as anarchy is central to the interpretation of Adam B. Ulam, who wrote, “The Bolsheviks did not seize power in this year of revolutions. They picked it up. First, autocracy, then democracy capitulated to the forces of anarchy. Any group of determined men could have done what the Bolsheviks did in Petrograd in October 1917: seize the few key points of the city and proclaim themselves the government.” *The Bolsheviks*, 314.

³³ “For all the lively debate and spirited give-and-take that I find to have existed within the Bolshevik organization in 1917, the Bolsheviks were doubtless more unified than any of their major rivals for power. Certainly this was a key factor in their effectiveness. Nonetheless, my research suggests that the relative flexibility of the party, as well as its responsiveness to the prevailing mass mood, had at least as much to do with the ultimate Bolshevik victory as did revolutionary discipline, organizational unity, or obedience to Lenin.” Rabinowitch, *The Bolsheviks Come to Power*, xxi.

On the question of mass backing for the Leninists, Rabinowitch has convincingly established that in the aftermath of the July Days the Bolsheviks did not lose as much support, particularly among workers, as many contemporaries and later historians believed. Their recovery was swift, thanks to "worsening economic conditions and the unpopular policies of the government and the majority of socialists."³⁴ The shift to the right by the Soviet was unpopular in many workers' districts, and the re-establishment of the death penalty at the front alienated the soldiers of the Petrograd garrison. The attempt by Kornilov to establish a military dictatorship raised industrial workers and soldiers from their summer lethargy to come out against the "counterrevolution." Kerensky was discredited in the eyes of the left and the masses for his involvement with Kornilov.

Even workers in industrial plants that heretofore had been Menshevik and Socialist Revolutionary strongholds, as well as soldiers in some of the politically restrained regiments of the garrison . . . , now turned against the government. . . . [Some of] the political resolutions passed at this time . . . called for the creation of a government representing workers, soldiers, and peasants; others, perhaps a majority, insisted on transfer of power to the soviets or creation of a revolutionary government responsible to the Soviet. . . . Common to virtually all were concern that Kornilov and his supporters be dealt with harshly so as to avoid further attacks by the "counter-revolution," aversion to political collaboration with the propertied classes in any form, and attraction for the immediate creation of some kind of exclusively socialist government which would bring an end to the war.³⁵

After the Kornilov Affair the possibility of collaboration with the liberals and upper classes, especially with the compromised Kadet party, had become anathema to the lower strata of the Petrograd population.

Rabinowitch's treatment of 1917 is still primarily political history, though not of the state but of an intermediate political organization with effective links to the lower classes. He has clearly demonstrated that the Bolshevik-Left SR positions on the war, opposition to the coalition government, and advocacy of a government by soviets were consonant with the aspirations of the lower classes in Petrograd. Most interestingly of all, he emphasized that the actual conquest of power in Petrograd by the Military Revolutionary Committee occurred a week before the fall of the Winter Palace. The question of who ruled in Petrograd was resolved by answering who had real control over the army in the city, and the decisive moment arrived when the Petrograd Soviet, in Bolshevik hands from early September, challenged the government's authority over the garrison. In the week before the Bolshevik insurrection, "the Military-Revolutionary Committee of the Petrograd Soviet took control of most Petrograd-based military units, in effect disarming the Provisional Government without a shot."³⁶ Once again, as in the first months of the revolution, the Soviet made it clear to the government that real power, the power to call troops into the streets and implement decisions, belonged to the Soviet. Formal power fell to the Soviet with the capture of the Winter Palace and the ratification by the Second Congress of Soviets.

³⁴ Rabinowitch, *The Bolsheviks Come to Power*, 90, 311.

³⁵ *Ibid.*, 158–59.

³⁶ *Ibid.*, 313–14.

Rabinowitch also took issue with the exaggeration of Lenin's role, so dominant a theme in the work of Melgunov and Daniels:

early on the morning of October 24, Kerensky initiated steps to suppress the Left. Only at this point, just hours before the scheduled opening of the Congress of Soviets and in part under the continuous prodding by Lenin, did the armed uprising that Lenin had been advocating for well over a month actually begin. . . . Only in the wake of the government's direct attack on the Left was an armed uprising of the kind envisioned by Lenin feasible. For . . . the Petrograd masses, to the extent that they supported the Bolsheviks in the overthrow of the Provisional Government, did so not out of any sympathy for strictly Bolshevik rule but because they believed the revolution and the congress to be in imminent danger.³⁷

Thus, although Lenin was instrumental in preparing the armed uprising, the actual social and political constellation of forces on October 24—most importantly the provocation by Kerensky—was responsible for the move into the streets.

Clearly, to isolate Lenin or his party from this rich and contradictory social context in which they operated not only distorts an understanding of the events of 1917 but may lead to unwarranted conclusions about the artificial, unorganic, manipulated nature of October and to the more general view that great revolutions, like more modest acts of political protest, are the creations of outside agitators. Although Rabinowitch has left many questions unanswered, including the critical one of the sources of the Bolsheviks' mass support, he has provided a persuasive reassessment of the personal and political factors that led to the October Revolution.

AT THE OTHER POLE OF HISTORICAL WRITING ON OCTOBER is *The Russian Revolution* (1976), the massive work of John Keep, a well-known specialist on Russian social democracy. Keep has focused on the mass organizations of the Russian people—the soviets, factory committees, trade unions, peasant committees, Red Guard units, and so forth—rather than on political parties or the government. Indeed, the book is quite difficult to follow if the reader does not have a firm grasp of the basic historical events of the period. There is, for example, no discussion whatsoever of the "April crisis" or the "July Days" and only the most cursory allusions to the *Kornilovshchina*. His approach seems, at first, to be diametrically opposed to the personality-dominated views of Melgunov and Daniels and also to the close political analysis of Rabinowitch. Yet the hundreds of pages spent on looking at the bottom of society reflect a peculiarly condescending view of the lower classes and reinforce the more traditional interpretations of the revolution.

Like so many other works on the Russian Revolution, Keep's book is history written backwards, from the results to their causes. He stated, "Any evaluation of this revolution's place in history must proceed from an awareness of the consequences to which it led: namely, the world's first experiment in totalitarian rule." And he sought material for this type of evaluation not so much in Leninist ideology or Bolshevik organization, though both clearly play a role, but in the methods and

³⁷ *Ibid.*, 314.

effects of mass mobilization. Keep has claimed that revolutions begin as anarchistic movements against the bureaucratic state but end up by setting up new bureaucratic organizations. "Chaos and anarchy . . . best describe the state of Russia during 1917."³⁸ Categories such as class interests or ideologies are not helpful, in Keep's view, in understanding the complexity of these events. More important is "instinct"! Of the crowds of workers in October. Keep noted, "the mob followed its instincts" in destroying things of value that it did not understand, or, later on, "the crowds followed a logic all their own, in which instinct was more important than reason."³⁹

The subrational, spontaneously generated political behavior of the workers is even more typical of the peasantry. "The agrarian movement," for Keep, "was neither a product of external agitation nor a manifestation of class struggle. It was a phenomenon *sui generis*—plebian, anarchic and anti-centralist." At times Keep showed little regard for the capacity of peasants and workers to understand the events and programs of 1917, as, for example, in his discussion of a gathering of trade unionists, where he stated, "Such intellectual subtleties were beyond the grasp of many delegates to the conference, let alone to their rank-and-file supporters." Or—"it is scarcely suprising that to the politically untutored Russian masses this propaganda [of the Bolsheviks] had a strong appeal." Yet, despite their lack of comprehension, workers in Keep's portrait were able, without intellectual leadership, to acquire some political consciousness, to arrive eventually at "a new awareness of their dignity as human beings, their rights as citizens and consumers." Indeed, "a class-oriented viewpoint came naturally to Russia's industrial workers," but Keep never explained why such a class outlook "came naturally."⁴⁰

Underlying and informing all of Keep's preferences and conclusions is a bedrock belief that ideological solutions to social and political problems are utopian, dangerous, and doomed to failure, that moderation and a spirit of compromise are the desired and most pragmatic political postures, that checks and balances are needed in government just as conciliation and cooperation are required in the economic conflicts between employers and employees. His great regret is that political democracy could not weather the storms of 1917, and his sympathies clearly lie with the right wing of the Soviet leadership, with men like Gots, Tsereteli, and Kerensky, rather than with Chernov, Martov, or Lenin. Yet in passages that echo Oliver Radkey, Keep castigated even these Russian socialist intellectuals, who were "brought up in a tradition that was ideological rather than pragmatic. . . . By instinct they were hostile to governmental authority even if the regime in question was one with which they themselves were associated."⁴¹ And, by their own manipulation of the working masses, Soviet moderates prepared the ground for the cruder direction of workers' affairs by the Bolsheviks.

Keep's explanation of the demise of soviet democracy is subtle and intriguing. The seizure of power was only one moment in a long process that began earlier in the revolution as real power shifted within the soviets from the plenary assemblies to cadre elements in executive positions. Autonomous soviets representing workers,

³⁸ Keep, *The Russian Revolution*, x–xi, 468.

³⁹ *Ibid.*, 214–56.

⁴⁰ *Ibid.*, 213–14, 108, 114, 27, 26.

⁴¹ *Ibid.*, 140.

soldiers, and peasants merged and were increasingly subjected to control from above. These tendencies "were in part spontaneous in origin and in part the deliberate outcome of official Bolshevik policy."⁴² As early as

the end of September the several hundred delegates attending meetings of the plenum could feel that they were shaping the country's destiny. Their aspirations seemed to accord so closely with the directives they received that they were scarcely conscious of being guided at all. The plenum of the Petrograd soviet became a genuine revolutionary assembly, a resonator automatically echoing the signals emitted from above.⁴³

Keep explained "this state of affairs . . . in terms not merely of crowd psychology but also of institutional mechanics." By the end of his analysis, the Soviet plenum becomes the chorus in a Greek tragedy: "blind instruments of fate, [the members] could scarcely follow the intricate maneuvers of their leaders upon which they were called to pronounce." The Military Revolutionary Committee of the Petrograd Soviet was "in essence a *junta* whose powers were defined solely by the ambitions of its leaders."⁴⁴

Three factors doomed the moderate socialists. First, the radicalism of many workers, especially metalworkers, which Keep has explained as the product of "their eagerness to renounce their close involvement in what seemed to them a disastrous and senseless war," played into the hands of the Leninists. Second, "the weakness of the Provisional Government obliged [the moderate socialists] to assume joint responsibility for direction of the nation's affairs, yet by doing so they inevitably became ever more isolated from their popular following." And, third, the skills of the Bolsheviks in conquering the working class and the soldiers through the soviets: "the soviets had never been conceived as organs reflecting the full range of their members' opinions; they were cadre organizations, whose purpose was to mobilize support for their leaders' policies. The Bolsheviks were simply more ruthless and systematic than their rivals in eliminating dissent by their long-familiar manipulative techniques."⁴⁵ Thus, radicalism from below, the class-oriented views of workers and others, and Bolshevik manipulation combined to turn the soviets into directors of popular affairs rather than reflectors of popular interests.

John Keep's "study in mass mobilization" is, in actuality, a study of mass manipulation by the radical intelligentsia. Evidently the Leninists, who "sought a complete monopoly of power for their own party," were the best of the manipulators. And this study of history at the bottom returns us full circle to the Melgunov-Daniels perspective. The mass organizations were simply the means through which the Bolsheviks worked to secure power; as "the soviets were converted from means of mass mobilization into instruments of party dictatorship," the masses of revolutionary Russia were shown to be the first dupes of the communists.⁴⁶

⁴² *Ibid.*, 340.

⁴³ *Ibid.*, 348. On this theme, also see Marc Ferro, "The Birth of the Soviet Bureaucratic System," in Elwood, *Reconsiderations of the Russian Revolution*, 100–32.

⁴⁴ Keep, *The Russian Revolution*, 348–49.

⁴⁵ *Ibid.*, 68–69, 140, 335–36.

⁴⁶ *Ibid.*, 447, 358. In Keep's explanation, both spontaneity and political action by the Bolsheviks contributed to their victory. The Bolsheviks were "being carried along on the tide of 'revolutionary spontaneity'"; this "bolshevization of the masses" to some extent came spontaneously, but "it was also to some extent the product of conscious political action by a segment of the intelligentsia"; *ibid.*, 350, 478. Keep has not denied the

In describing at a distance the shift to the left of Petrograd's masses, Keep did not provide much of an explanation for their radicalization, except—as did Melgunov and Daniels—through an appreciation of the superiority of Bolshevik will and skill. What seems at first to be a “social history with the politics left out” might be characterized more accurately as “political history disguised as social history,” for there is no feel in *The Russian Revolution* for the coincidence of workers' aspirations and Bolshevik ideals in the context of deepening social polarization. For this aspect of the picture we must turn to those historians who have studied the working class, the army, and the navy. Although the works under review do not individually present a complete synthesis of social dynamics and political developments, through such works Western historians are providing the raw material for a new paradigm to explain the “deepening of the revolution” and the victory of the Bolsheviks in October.

SEVERAL WESTERN HISTORIANS HAVE MADE MUCH of the workers' instinctive rebelliousness or propensity for anarchy; but the real and incipient violence in the revolution does not necessitate concluding, as John Keep has argued, that workers were instinctively distrustful of authority in any form.⁴⁷ Given their long and painful experience with arbitrary authority under the Romanovs, workers were understandably concerned about the proper exercise of power, and they searched for forms of self-organization and demanded authorities responsive to their needs. The growing feeling that the Provisional Government, even in its coalition variant, was not a responsive authority led to workers gravitating to soviet power. This sense by workers of their own interests being frustrated by hostile middle and upper classes can be seen both as a revival of prewar attitudes of the most militant workers and as a reaction to perceived counterrevolutionary attitudes and actions by industrialists, intellectuals (both liberal and socialist), and, in time, the government.

Among historians who have looked most closely at workers' activities, the superficial impression of chaos has been superseded by the realization that workers' actions in 1917 demonstrate a “cautious and painful development of consciousness” that was “an essentially rational process.” After distinguishing three principal strata of workers—the politically aware skilled workers (primarily the metalworkers of the Vyborg district), the unskilled workers (largely women textile workers), and the “worker aristocracy” (characterized best by the pro-Menshevik printers)—David Mandel has shown that the metalworkers were most radical in the political sphere, calling for the early establishment of soviet power, while the unskilled workers, who tended to be more moderate on political issues, showed the greatest militancy in the struggle for higher wages.⁴⁸ The contours of worker activity are complex but not

importance of Bolshevik ideas and their resonance in the populace, but he has argued that “the organization devices they employed” were essential: “On the Russian Left the Bolsheviks alone—or to be more precise, those identified with the ‘hard’ Leninist positions—possessed the will and experience to turn vague and transient mass attitudes into firm political commitments”; *ibid.*, 380–81.

⁴⁷ Keep, *The Russian Revolution*, 77.

⁴⁸ Mandel, *The Petrograd Workers and the Fall of the Old Regime: From the February Revolution to the July Days, 1917* (London, forthcoming), and *Petrograd Workers and the Soviet Seizure of Power (July 1917–June 1918)* (London, forthcoming).

chaotic. Stephen Smith has broken down the metalworkers into shops and carefully delineated between “hot” shops, such as foundries, where most newly arrived peasant-workers were located, and “cold” shops, such as machine shops, where highly skilled and literate workers were most receptive to social democratic activists. In examining the Putilov Works, Smith found that workers there moved more slowly toward Bolshevism than in other metalworking plants and that “shopism” and “conciliationism” remained stronger there than elsewhere.⁴⁹ Both Smith and Mandel have presented pictures of growing worker suspicion of the upper levels of society, especially after the formation of the coalition government—a suspicion that translated into struggles for control in the factories and increased opposition to those moderate socialists who were backing the government.

Although the rapidity of labor radicalization in Petrograd is certainly distinctive, similar processes, marked by growing class cohesion and consciousness, were evident in other parts of the country, as my own work on Baku and Donald Raleigh’s study of Saratov demonstrate.⁵⁰ By engaging in a detailed quantitative analysis of the dynamics of labor activity in Moscow, Diane Koenker has also reached a similar conclusion:

One must . . . reject the image of the Russian working class as uniformly irrational, poorly educated, and incapable of independent participation in the political process. One must reject in particular the myth that the revolution in the cities was carried out by dark semipeasant masses “who did not understand the real meaning of the slogans they loudly repeated.” Yes, of course, many Moscow workers were more rural than urban; but when one looks at the participation levels of different segments of the urban labor force, the fact that skilled urban cadres, not the unskilled peasant mass, were the leading political actors can be seen over and over again. These workers possessed experience, political connections, and the degree of economic security which enabled them to function freely and easily in the political life of 1917.⁵¹

To Koenker, the radicalization of workers in the first year of the revolution was an “incremental process, which took place in response to specific economic and political pressures.” Her conclusion is supported in other studies as well. Galili y Garcia, for example, has noted the delayed radicalization of the less politically conscious, unskilled workers in the second quarter of 1917, observing that these less well-organized workers had not benefited from the initial round of wage raises in March and April. By the time they made their bid for higher pay, the industrialists

⁴⁹ Smith, “Craft Consciousness, Class Consciousness,” 36–37. The central argument of Smith’s article coincides with Mandel’s view of growing worker militancy and class consciousness. Smith has pointed out that “shopism” and “factory patriotism” did not preclude labor militancy or inhibit “the development of a broader sense of belonging to a class of working people whose interests were antagonistic to those of the employers”; *ibid.*, 51.

⁵⁰ Suny, *The Baku Commune, 1917–1918*; and Raleigh, “The Russian Revolutions of 1917 in Saratov.” In Baku, economic collapse and food shortages stimulated workers to rely less on their soviet leaders and to take actions into their own hands. Particularly important in their growing impatience was their sense that both industry and government were unnecessarily delaying the signing of a labor contract for the oil workers. The general strike of September 1917 was initiated by workers in factory committees against the advice of the socialist parties, including the Bolsheviks. See “From Economics to Politics,” chapter 4 of Suny, *The Baku Commune, 1917–1918*, 102–46.

⁵¹ Koenker, *Moscow Workers and the 1917 Revolution*, 360.

had adopted a more intransigent attitude.⁵² By mid-May there were already forty thousand unemployed workers in Petrograd, and as real wages began to plummet and mass dismissals accelerated, more and more less-skilled workers joined the “proletarians” in a commitment to soviet power. By June–July a majority of Petrograd workers were already opposed to the coalition government and shared a sense of separate and antagonistic interests between workers and the propertied classes. A greatly heightened sense of class was apparent among the mass of workers by the summer.

The studies of Mandel, Galili y Garcia, Koenker, and others provide the specifics of the economic and political stimuli that led to radicalization, and for the first time it has become possible to understand how individual grievances within the larger context of social polarization combined to create class antagonisms. Given that Russia’s workers had long been closely involved with a radical, socialist intelligentsia anxious to forge a Marxist political culture within the urban labor force, it is hardly surprising that workers in 1917 should “naturally” come to a “class-oriented viewpoint.” Koenker summed up this development in her conclusion, which gives us social history with the politics left in:

That the revolutionary unity of March fell apart along class lines can be attributed to economic conditions in Russia but also to the fact that the class framework was after all implicit in socialist consciousness. Capitalists began to behave as Marx said they would: no concessions to the workers, no compromise on the rights of factory owners. Mensheviks and SRs tried to straddle both sides of the class split; this appeal can be seen in the mixed social composition of their supporters. The Bolsheviks, however, had offered the most consistent class interpretation of the revolution, and by late summer their interpretation appeared more and more to correspond to reality. . . . By October, the soviets of workers’ deputies, as the workers’ only class organ, seemed to class-conscious workers to be the only government they could trust to represent their interests.⁵³

The perception by workers of common interests with fellow workers and of shared antagonisms toward the rest of society was complemented by a growing sense of hostility from the upper levels of society toward the lower classes. William G. Rosenberg has illustrated this shift to the right by the Kadets as the liberals’ growing identification with commercial and industrial circles changed them from a party of liberal professionals and intellectuals into Russia’s party of the bourgeoisie.⁵⁴ Even as they persisted in maintaining their “nonclass” ideology, the Kadets emerged as the de facto defenders of a capitalist order and the determined opponents of the approaching social revolution desired by the more militant of the *nizy* (“lower classes”). Their isolation from the socialist workers and soldiers led the liberals to turn to the military as a source of order and power. Rosenberg has argued, as had the Left Kadets in 1917, that the only hope for a democratic political outcome in Russia was lost when the Kadets failed to work effectively with the moderate socialists in the coalition government and to make significant concessions to the

⁵² *Ibid.*, 363–64; and Galili y Garcia, “The Menshevik Revolutionary Defensists and the Workers,” chap. 5: 43–62.

⁵³ Koenker, *Moscow Workers and the 1917 Revolution*, 364.

⁵⁴ Rosenberg, *Liberals in the Russian Revolution*, 31, 154–55.

lower classes. "The very coalition with moderate socialists that Miliukov and the new tacticians strove for so persistently in emigration [after the Civil War] *was* possible in the summer of 1917."⁵⁵ The failure to form such a liberal-socialist alternative to Bolshevism might be seen as the consequence of the Kadets' lack of "true liberal statesmanship," but Rosenberg's analysis permits an alternative explanation.⁵⁶ As the Kadet party evolved into the principal spokesman for propertied Russia, it was increasingly unlikely to compromise the interests of the privileged classes that backed the party in order to form a dubious alliance with the *demokratiia*, whose ever more radical demands threatened the very existence of privilege and property. *Nadklassnost'* (the Kadet notion of standing above class considerations) was simply a utopian stance in a Russia that was pulling apart along class lines.

Against this background of deepening social cleavage with all its inherent fear and suspicion, hope and despair, the question of power was posed in the summer and fall of 1917. Underestimating the extent of the social polarization, the growing intensity of class hostility, and the perceived irreconcilability of the interests of the *demokratiia* and the *tsenzovoe obshchestvo* within the constraints of the February regime leads historians away from any satisfactory explanation of the failure of the moderates and the victory of the Bolsheviks, forcing them to rely on accidental factors of will and personality. Only through a synthesis of political and social history in which the activity and developing political consciousness of workers, soldiers, and sailors is taken seriously can the Bolsheviks' success be adequately explained.

BY THE SUMMER of 1917 there were four possible solutions to the problem of who would rule Russia. The first solution was that advocated by Tsereteli and Kerensky, by the Menshevik Revolutionary Defensists and the Right Socialist Revolutionaries: a continuation of the coalition government, a policy of social unity and class collaboration to defend Russia against her enemies and prevent civil war. But such a solution was doomed in the face of the deepening social crisis and political paralysis. Given the hostility between classes and their mutually antagonistic aspirations and interests, a coalition government could move neither to the left nor to the right without stirring up opposition. It could neither satisfy the demands of the peasants for the land nor attempt to protect the landlords' rights to private property. Paralyzed between its competing constituencies, all movement looked like vacillation, the product of a lack of will or determination, but was in fact the result of the real political bind faced by a government stretched between the extremes of a splintering society.

A second solution was a government made up of the upper classes alone—that is, a dictatorship of the center and the right. The Kadet leader Miliukov had desired such a government at the beginning of the revolution, but, without a base of

⁵⁵ *Ibid.*, 469.

⁵⁶ For a fuller exposition of my pessimism concerning a liberal-moderate socialist coalition in 1917, see "Some Thoughts on 1917: In Lieu of a Review of William Rosenberg's *Liberals in the Russian Revolution*," in *Sbornik: Papers of the First Conference of the Study Group on the Russian Revolution* (Leeds, 1975), 24–27.

support in the population, that hope collapsed finally in the April crisis. Real power was in the hands of the Soviet; as Stankevich quipped, "The soviet could make the Provisional Government resign with a telephone call."⁵⁷ The only possible way for the upper classes to rule—and the liberals as well as the right came to this conclusion by mid-summer—was to establish a military dictatorship. Kerensky and Kornilov worked toward that goal in August, but it ended in Kornilov's ill-fated grasp for power, thwarted not just by his disagreement with Kerensky over the final disposition of power but by the actions of workers, soldiers, and soviets. With the failure of the military *coup d'état*, the only possibilities remaining were for a government by one or several of the Soviet parties.

The third solution—and probably the one most desired by the lower classes in urban Russia—was an all-socialist regime (*odnorodnoe sotsialisticheskoe pravitel'stvo*), a government representing the workers, soldiers, and peasants of Russia but excluding the *tsenzovoe obshchestvo*. A broader variant of this solution, the *odnorodnoe demokraticheskoe pravitel'stvo*, would have included nonsoviet "democratic" elements such as municipal and government workers, people from cooperatives, and small shopkeepers. Historians seem to agree that, when workers and soldiers voted for soviet power, they were in fact opting for a multiparty government of the leftist parties. This solution was never really implemented because of the serious divisions between the moderate socialists and the Bolsheviks, and there is legitimate doubt that the former defenders of coalition and the advocates of working-class rule could have lasted long in an all-socialist coalition.

In October 1917 the Bolsheviks came to power in the name of the soviets. The coalition government had been completely discredited, and almost no one would defend it in its last hours. The Military Revolutionary Committee merely completed the process of political conquest of the population of Petrograd that the Bolsheviks had begun months earlier.⁵⁸ Bolshevik policy, alone among the political programs, corresponded to and reflected the aspirations and perceived interests of workers, soldiers, and sailors in Petrograd. Even without much direct participation in the October Days, the workers acquiesced in and backed the seizure of power by the soviets. Lenin and a few others had seen the potential in 1917 for a government of the lower classes. He, earliest among almost all of the political leaders, had understood that in this revolution—marked by deep, long-standing social tensions and stoked by a seemingly endless war—unification of "all the vital forces of the nation" became increasingly unlikely. By late spring an all-class or nonclass government was no longer possible, and by linking his party's fortunes with the real

⁵⁷ Stankevich, as quoted in David S. Anin, "The February Revolution: Was the Collapse Inevitable?" *Soviet Studies*, 18 (1967): 448. For Stankevich's original statement, see I. G. Tsereteli, *Vospominaniia o fevral'skoi revoliutsii*, 1 (Paris-la-Haye, 1963): 97.

⁵⁸ Soviet historians estimate that the actual balance of forces in Petrograd on the eve of the insurrection involved about three hundred thousand armed supporters of the Bolsheviks and only about twenty-five thousand ready to fight for the Provisional Government. At the storming of the Winter Palace, the Bolshevik Red Guards, soldiers, and sailors numbered approximately twenty thousand, with defenders of the palace estimated at about three thousand. In the fighting from October 24 to 26, less than fifteen people were killed and less than sixty wounded. Roy A. Medvedev, *The October Revolution*, trans. George Saunders (New York, 1979). Medvedev based his figures on E. F. Erykalov, *Oktiabr'skoe vooruzhenoe vosstanie v Petrograde* (Leningrad, 1966), 303–04, 434–35, 461, 462.

social movement in Petrograd he was able to destroy the flimsy coalition of liberals and moderate socialists and to facilitate a seizure of power in the name of the soviets.

Instead of soviet power or socialist democracy, however, the Russian people eventually received a dictatorship of the Bolshevik party. This was the fourth possible solution to the question of power. Why it succeeded over the more democratic third solution is a question that goes beyond the limits of this essay, for the answer in this instance lies not so much in the events of 1917 as in the long years of the Civil War. But again, the answer cannot be provided by resorting to personal and ideological influences or by extending the analysis based on politics alone; rather, it lies in an examination of the intense class struggle that was carried beyond the limits of the city of Petrograd into the countryside and all the provinces of Russia.

AS HISTORIANS HAVE SHIFTED THEIR ATTENTION away from the political elites that formerly dominated explanations of the Russian Revolution and to the people in the streets, the victorious Bolsheviks have appeared less like Machiavellian manipulators or willful conspirators and more like alert politicians with an acute sensitivity to popular moods and desires. Without forgetting the authoritarian traits in Lenin, Trotsky, and other Bolshevik leaders or the power of the image of the party outlined in *Chto delat'?*, we can move on to a new paradigm for understanding 1917 that reduces the former reliance on party organization or personal political skills so central to older explanations. The key to a new paradigm is an appreciation of the deepening social polarization that drew the upper and middle classes together and away from the workers, soldiers, and peasants. The *tsenzovoe obshchestvo*, after an initial period of compromise in the early spring of 1917, began to resist encroachments on its prerogatives in the economy and in foreign policy and, faced by growing militancy among workers and soldiers, developed a clearer and more coherent sense of its own political interests. Likewise, the workers and soldiers, confronted by lockouts, falling wages, a renewal of the war effort, and perceived sabotage of the revolution as they hoped it to be, evolved their own sense of class interests. In time, both parts of Russian society found their interests to be incompatible, and those parties that tried to stand "above class" or to unite "all the vital forces of the nation" were either compelled to take sides with one major force or abandoned by their former supporters.

For Russians in 1917, the revolution was a struggle between classes in the inclusive sense of the *verkhi* ("upper classes") versus the *nizy* ("lower classes"). Those broad, antagonistic "classes" coalesced in the course of 1917. This heightened feeling of class was forged in the actual experience of 1917 and contained both social hostilities bred over many years and intensified under wartime and revolutionary conditions and a new political understanding that perceived government by soviets as a preferable alternative to sharing power with the discredited upper classes. The Bolsheviks had since April been advocating such a government by the lower classes. With the failure of the coalition and its socialist supporters to deliver

on the promise of the revolution, the party of Lenin and Trotsky took power with little resistance and with the acquiescence of the majority of the people of Petrograd. The Bolsheviks came to power not because they were superior manipulators or cynical opportunists but because their policies as formulated by Lenin in April and shaped by the events of the following months placed them at the head of a genuinely popular movement. Sadly for those who had overthrown autocracy and turned to the Bolsheviks to end the war and alleviate hunger, that solution based on a government by soviets evolved inexorably through a ferocious civil war into a new and unforeseen authoritarianism.

Review Essay

Guilt, Redemption, and Writing German History

THEODORE S. HAMEROW

NEARLY FORTY YEARS AFTER ITS COLLAPSE, the Third Reich remains the central historical experience of modern Germany, a reference point against which past and present continue to be measured. To an even greater extent than 1789–1814, 1858–71, or 1914–18, the period 1933–45 is regarded as the watershed between an *ancien régime* and a new order, separating distinct political philosophies, civic ideals, social values, and national loyalties. That is why scholarship has continued to explore the dimensions of German totalitarianism, not only its course and outcome but also its origins and background. The Nazi dictatorship attracts our attention by its unpredictable turns and dramatic contrasts: a country sunk in defeat recovering to dominate Europe, a people renowned for contributions to culture engaging in acts of barbarism, military success of unparalleled scope turning into unconditional surrender. How could all this happen? That is the question historians have been trying to answer, producing a vast scholarly literature unmatched in intensity or detail. The flood shows no sign of receding. New works continue to appear year after year, all seeking to enlarge our understanding of why the spirit of Weimar could not prevent the reality of Auschwitz. The period 1933–45 remains the culmination of German history.

Here and there, however, a few historians have suggested that we are devoting too much attention to the Third Reich, that the time has come to take a broader view in which the Nazi regime would be seen as part of a continuous historical process extending beyond the capture of the *Führerbunker*. Just over a decade ago Geoffrey Barraclough argued in the pages of the *New York Review of Books* “that we have gotten about as far as we are likely to reach along the road most historians have trodden since 1945, and that the time has come for new directions and new goals.” He conceded that the preoccupation of scholars with the Nazi experience had produced a mass of information unequalled for any other period of history, but “whether it has brought more understanding is another question.”¹

We should now be asking ourselves, Barraclough maintained, whether the effort expended on further research will really be worth the results obtained, whether “historians are not starting to scrape the bottom of the barrel.” The hunt for the

¹ Barraclough, “Mandarins and Nazis,” *New York Review of Books*, October 19, 1972, p. 37.

"roots" of the Third Reich had led to a scrutiny of the entire period from 1871 to 1945 for the cloven hoof of National Socialism, a scrutiny inspired "as much by indignation, resentment, and *parti pris* as by a disinterested search for historical truth." English and American scholars had found portents of the Third Reich at every turn of modern German history; German historians of the older generations had insisted that Hitler's seizure of power was a misfortune that could befall any country; left-wing writers had described National Socialism as a tool of German capitalism; right-wing writers had seen in it a movement of the ignorant masses mesmerized by Hitler's demagoguery. "When the dust settles, the verdict on these controversies is likely to be that they engendered more heat than light."²

Barracrough conceded that the question of continuity is highly complex, but to him the view that 1945 created a chasm in German history is "a misleading simplification." When a coherent, critical account of the history of Central Europe during the last hundred years comes to be written, he was sure, this question would play a very important part. For the Nazi years would then have to be considered not only as a breach of continuity, which they admittedly were in some respects, but also as "a stage on the road from the Germany of Bismarck to the Germany of Brandt." The first requirement for a clearer perspective on German history was, therefore, a new chronological frame that begins in 1871 but does not stop short in 1945. The "liberal" periodization of German history, the periodization that depicts the fall of the Third Reich as a decisive turning point, reflects a view of the past that we cannot accept and an outlook that we no longer share. "It is geared to political events, puts more emphasis on legal and constitutional forms than on social and economic relationships, and makes the national state its criterion." The need for a new examination of Germany's past was beyond dispute, for the outworn liberal interpretation had been loaded with "the burden of guilt and the emotional associations of 1945."³ The time had come for a more objective and comprehensive analysis.

Barracrough's call for a revised interpretation of the Third Reich was followed by suggestions regarding the direction that it should take. After all the bold rhetoric concerning conventional historiography, his remarks sound a little tame and anticlimactic. Repeating that the accepted "liberal" view of 1945 as an "absolute zero" was misleading, he went on to explain that

defeat was certainly more of a breach of continuity in 1945 than in 1918, but the German capitulation was only one point in a twilight period that began in June or July, 1944, and continued until 1948 or 1949. Once again, as after the First World War, it was economic rehabilitation—the currency reform of June, 1948—that marked the beginning of a new period. This time it was accompanied by the division of Germany and the setting up of the Federal and Democratic Republics in 1949.⁴

That was the sum of the brave new approach to the historical meaning of the Nazi regime.

² *Ibid.*, 37–38.

³ Barracrough, "The Liberals and German History," *New York Review of Books*, November 2, 1972, pp. 34–35, and "A New View of German History," *ibid.*, November 16, 1972, pp. 25, 31.

⁴ Barracrough, "A New View of German History," 29.

IT IS HARD TO SEE, EVEN AFTER CAREFUL REFLECTION, why the view that the abortive attempt to assassinate Hitler in 1944 marked the end of one period in the history of Central Europe and the beginning of another, which continued until the formation of the two Germanys in 1949, should be regarded as superior to the more traditional notion that the decisive break came in 1945 with the unconditional surrender of the Third Reich. Such a drastic revision of historical periodization does possess the virtue of originality, but at the expense of good judgment and common sense. Historians have therefore continued to accept the centrality of the period 1933–45, recognizing its significance as a reflection of political, social, or cultural forces deeply rooted in the life of Central Europe. Their differences have concerned the nature and origin of those forces, not their importance or chronology. For half a century now scholars have regarded the Nazi dictatorship as a valid field of historical study, valid in its own right by virtue of its far-reaching consequences, not as part of a larger whole. This view shows no sign of changing.

What has changed is the way historians have interpreted the meaning of the Third Reich. To those who first dealt with the question in the years after 1945, the main task was to explore those ideas and beliefs running through German history that led to National Socialism. From its beginning the study of the Third Reich was thus bound up with the issues of guilt and responsibility—responsibility for tyranny, aggression, and genocide. How could it be otherwise? The scholars of that generation were still living in the shadow of the Second World War. They could remember the years of triumph of the Nazi dictatorship; many had taken part, on one side or the other, in the long military struggle that had destroyed the prewar world of their youth. For them the attempt to assess the historical significance of the Third Reich was linked with personal memories and private experiences. They could not achieve the objectivity that a scholar ordinarily acquires through distance, detachment, and a certain indifference.

What the historians grappling with the problem of National Socialism had to say during the 1940s and 1950s was bound to reflect how they regarded not only the past but the future of the Germans. Had they been dupes or accomplices of the Hitler dictatorship? Had they been ignorant of or merely unmoved by the atrocities it had committed? Had their acceptance of the Third Reich been the result of sudden misfortunes—a lost war, a humiliating peace, a ruinous inflation, a cruel depression—or of powerful secular forces that could be traced back for more than a hundred years? Could they now be trusted to rebuild their national life in accordance with democratic principles, or would they revert to type as soon as they had a chance? Such were the questions implicit in the work of the first generation of scholars to deal with the Third Reich.

These scholars can conveniently be divided into accusers and defenders, those who maintained that the roots of National Socialism were deeply embedded in modern German history and those who argued that the Hitler regime represented the corruption of a people whose national tradition was no worse than most. The former came from countries that had fought against the Axis during the Second World War, primarily the United States but also England and France. Many of them were refugees from Germany, driven out of their homeland by ethnic or

political bigotry, now sitting in judgment on those at whose hands they had suffered humiliation and persecution. The common characteristic of the accusers was the belief that National Socialism had emerged gradually but logically from established traditions in German life, which found fulfillment in 1933. Hitler was not an aberration but a culmination. He came to power on a tide of romantic nationalism, which had originated in the late eighteenth or early nineteenth century. The task of scholarship was to unravel the skein of ideals, loyalties, and beliefs extending from Frederick the Great and the War of Liberation to the concentration camps and gas chambers of the Third Reich. This approach to Central Europe had its source in the prewar and wartime years as a reaction against the barbarism of the Nazi regime, but it did not reach its full development until after 1945.

The American historian Peter Viereck maintained that the German people began to stray when they turned away from the cosmopolitan restraint of conservative statesmen like Metternich to romanticism and nationalism. This was the start of the road that led a misguided nation to the Hitler dictatorship. "Is it being unhistorical to judge the anti-Metternichian nationalism and racism of 19th-century Germany by its Nazi consequences?" he asked. "Were these consequences the logical outcome or a modern accident for which nationalism should not be blamed? Is it a case of the wise-after-the-event fallacy to read so much into these early rebels of 1806–1848, whom many historians still consider great liberals?" For Viereck's readers there could be no doubt about the answer. Those who helped spread the gospel of national unification, especially in the institutions of higher learning, prepared the way for the corruption of the German people.

The liberal university professors, Metternich's fiercest foes and now so prominent in 1848, were often far from the cloudy idealists pictured in our textbooks. From his own viewpoint, Bismarck erred in mocking their lack of *Realpolitik*. The majority . . . was more Bismarckian than Bismarck ever realized. Many liberals . . . later became leading propagandists for Bismarck, along with the new National Liberal Party. Only an honorable few continued to oppose him and the militarist success-worship that followed his victorious wars.⁵

Once blood and iron prevailed over legitimacy and tradition, it was only a question of time before the brown shirts took over.

Edmond Vermeil in France agreed with this interpretation, although his emphasis rested on the period after rather than before 1871. The Third Reich, he wrote, despite the abnormal character of the circumstances that preceded it, was not "a purely adventitious episode appearing on the fringes of the German tradition." To understand it, we must examine the spread of a restless nationalism in Central Europe, beginning with the wars of the French Revolution and the Napoleonic empire, but especially after the dismissal of Bismarck. For the Iron Chancellor, by creating the German empire, had opened for his country unexpected new prospects, although he had been able to restrain its impetuous drive. "It was after his fall, under William II, that this nationalism, breaking all barriers and escaping from the grip of a weak government, gave rise to a state of mind and a general situation that we have to analyze, for otherwise Nazism with its momentary

⁵ Viereck, *Conservatism Revisited: The Revolt against Revolt, 1815–1949* (New York, 1949), 59, 73.

triumphs and its terrible collapse will remain incomprehensible." Such an analysis must lead to the conclusion that the German nation, which had now survived two great catastrophes, would continue to follow a separate path, "always placing the spirit of its implacable technical discipline at the service of those visions of the future that its eternal romanticism begets."⁶ Only the vigilance of the other states of Europe could check the danger of German irredentism.

The same theme appeared in the work of British scholars during the early postwar years. In his Raleigh Lecture on History, read before the British Academy in the summer of 1944 and published two years later, Lewis B. Namier argued that the nationality conflicts in Central Europe during the revolutions of 1848–49 had already revealed the German combativeness that the history of the next hundred years amply confirmed. He spoke of "the early manifestations of aggressive nationalisms, especially of German nationalism which derives from the much belauded Frankfort Parliament rather than from Bismarck and 'Prussianism.'" By examining the relation of the German liberals, "in reality forerunners of Hitler," to the Poles and Czechs, he touched on problems that in 1938–39 became once more "a touchstone of German mentality and a decisive element in East-European politics." There were differences between the two periods, to be sure. After the "springtime of peoples," the old order had defeated its opponents, thereby saving the reputation of German liberalism and preventing the "revolution of the intellectuals" from consummating *la trahison des clercs*. And yet, "had not Hitler and his associates blindly accepted the legend which latter-day liberals, German and foreign, had spun round 1848, they might well have found a great deal to extol in the *deutsche Männer und Freunde* of the Frankfort Assembly."⁷ Here again is the scarlet thread tying the brutalities of the Nazi dictatorship to the historical development of the German people.

Indeed, to A. J. P. Taylor the Third Reich was the most authentically German of all the forms of authority the Germans had known. Though founded on terror and unworkable without the secret police and the concentration camp, the Nazi regime also "represented the deepest wishes of the German people." It was the only German government ever created by German initiative. The Holy Roman Empire had been imposed by Austria and France, the German Confederation by Austria and Prussia, and the Weimar Republic by the Allies. "But the 'Third Reich' rested solely on German force and impulse; it owed nothing to alien forces. It was a tyranny imposed upon the German people by themselves."⁸

Many Germans, most perhaps, were reluctant to make the sacrifices that rearmament and total war demanded, Taylor conceded, but they sought a prize only total war could give them. They wanted to reverse the verdict of 1918, not only to end reparations and reject the charge of war guilt but also "to repudiate the equality with the peoples of eastern Europe which had then been forced upon

⁶ Vermeil, *L'Allemagne contemporaine: Sociale, politique, et culturelle, 1890–1950*, 2 vols. (Paris, 1952–53), 1: 301, and 2: 325, 348–49.

⁷ Namier, "1848: The Revolution of the Intellectuals," *Proceedings of the British Academy* (1944), 191, 281–82.

⁸ Taylor, *The Course of German History: A Survey of the Development of Germany since 1815* (London, 1945), 213.

them.” Here Taylor returned to the theme that Namier had developed in his lecture on 1848:

During the preceding eighty years the Germans had sacrificed to the Reich all their liberties; they demanded as a reward the enslavement of others. No German recognized the Czechs or Poles as equals. Therefore, every German desired the achievement which only total war could give. By no other means could the Reich be held together. It had been made by conquest and for conquest; if it ever gave up its career of conquest, it would dissolve.⁹

Patriotic duty thus forced even the best of Germans to support a policy that was leading their country to disaster.

The scholars presenting the case for the prosecution relied primarily on the force of ideas to explain the continuity of German history. The link between the Third Reich and the Weimar Republic, the German empire, the revolutions of 1848–49, or the War of Liberation was the persistence of beliefs, attitudes, and loyalties that shaped political institutions, military objectives, and social relations. The concepts of authoritarianism, nationalism, romanticism, militarism, and racism possess a causative autonomy that enable them to determine the collective actions of a people from generation to generation. Before 1945 the historiography on Germany had to a large extent rested on the *Primat der Aussenpolitik*, the primary importance of foreign affairs. Since 1965 it has tended in the direction of the *Primat der Innenpolitik*, the crucial role of internal developments. But for the first postwar generation of scholars the underlying assumption was what might be called the *Primat der Geistespolitik*, the decisive influence of the intellect, of cultural forces and ideological currents, as the determinant of historical development. Just look at the titles of some of the best-known works published in the United States at that time explaining how the Germans got the way they are: *The German Idea of Freedom* (1957), *The Mind of Germany* (1960), *The Politics of Cultural Despair* (1961), *The Crisis of German Ideology* (1964). The key to an understanding of what happened during the Third Reich, they suggest, lies not in political institutions or social relationships but in terribly wrong ideas that helped mold politics and society.

Thus, in the preface to his thoughtful, scholarly study of how the Germans have regarded the concept of liberty from the Reformation to the German empire, Leonard Krieger asked, “Did the Germans’ failure to achieve, under their own power, a liberal democracy in the western sense mean simply the triumph of conservatism over generic liberalism in Germany or was a peculiar German attitude toward liberty involved in its defeat?” A perceptive reader could guess the answer. There was indeed a peculiar German attitude toward liberty, it turned out, which derived from an “age-old association of liberty with the authoritarian state.”¹⁰

Hans Kohn published a less extensive study of ideological currents in Central Europe, beginning with the Enlightenment rather than the Reformation, but his findings are in important respects similar to Krieger’s. Although the Germans have enriched the intellectual heritage of mankind with contributions equal in a number of fields to the greatest achievements in history, “they have developed attitudes

⁹ *Ibid.*, 213–14.

¹⁰ Krieger, *The German Idea of Freedom: History of a Political Tradition* (Boston, 1957), ix, 468.

which separated them from the main trends characteristic of modern western civilization." They have offered the world what is perhaps the most outstanding example in our times "of the moral catastrophe, into which the malady of a self-centered nationalism and the accompanying *trahison des clercs* can lead." Hitler appealed to sentiments deeply rooted in German history, unchecked by "the liberal-humanitarian considerations Western Europe inherited from the Enlightenment." The source of the divergence of Germany from England and France lay in the romantic movement, which rejected the restraints imposed by classicism. In Central Europe romanticism, although it may at first have been apolitical, "encouraged the development of German nationalism after 1800 and influenced it as much as the Enlightenment shaped the form of nationalism in Western Europe."¹¹

For Fritz Stern and George L. Mosse the turning point in the political thought of Central Europe came a little later, after the founding of the German empire. There may have been warning signs before, but, for a systematic study of the "rise of the Germanic ideology" or the "intellectual origins of the Third Reich," we must look to the period after 1871. Stern contended that the ideas of Paul de Lagarde, Julius Langbehn, and Arthur Moeller van den Bruck had strongly affected the sentiments, the *Lebensgeföhle*, of respectable Germans for two generations before Hitler. "These *idées-forces* remained, to be sure, a subterranean force, an undercurrent of belief, visible only in moments of crisis. But they nurtured the idealistic rejection of modern society and the resentment against the imperfections of Western ideals and institutions, that contributed so greatly to the debility of democracy in Germany." The reason why the "conservative revolution," which was a European phenomenon, became a decisive intellectual and political force only in Germany is that "this particular reaction to modernity was deeply embedded in German thought and society, and . . . this curiously idealistic, unpolitical discontent constitutes the main link between all that is venerable and great in the German past and the triumph of national socialism." In short, "long before Hitler, long before Versailles, there appeared in Germany deep national frustrations, galling cultural discontents, which inspired nationalist fantasies and utopias which found ready assent among [the] German elite."¹² Even without Hitler and the succession of disasters resulting from the First World War, their presence must be taken into account in an examination of the political culture of modern Germany.

Mosse's study of the period after 1871 leads to the same conclusion. What differentiated Germany from other nations was a "peculiar view of man and society which seems alien and even demonic to the Western intellect." To find an explanation of the unique development of Central Europe, we must understand the growth of the ideas of "racial thought, Germanic Christianity, and the Volkish nature mysticism." We must understand the role they played and the longings they gratified. Historians have tended to ignore them, regarding them as a species of subintellectual rather than intellectual history. Yet their importance should not be overlooked, for this complex of particularly German values and ideas conveyed the

¹¹ Kohn, *The Mind of Germany: The Education of a Nation* (New York, 1960), ix-x, 9, 49.

¹² Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley and Los Angeles, 1961), xiv, xxiii, 291-92.

great issues of the last hundred years to important segments of the population. The deviation of German thought from that of the West also explains the difference between German fascism and the other fascist movements. Because of its peculiar ideology, "this unfortunate nation" repudiated a European heritage that was still alive elsewhere, a heritage derived from the rationalism of the Enlightenment and the social radicalism of the French Revolution. The repudiation was connected with a general opposition to modernity. The "awakening of German individuality" was actually an escape from present reality, a flight from common tradition. By examining it, we can discern how the German form of fascism differed from those of Western Europe, for, while they tried to reach out to each other, National Socialism did so only in order to "further subversion on foreign soil for its own ends, not out of a deeply felt longing for a larger unity."¹³

To summarize, the historians who argued that the Third Reich must be seen as part of a historical continuity in Central Europe identified its source as a complex of ideas regarding authority, warfare, state, society, race, and *Volk*, which had in the course of time perverted a people. Their effect could be seen at every turn in the development of modern Germany, culminating in the tragedy of National Socialism. But, although the sins committed by the Germans were grave, the road to their redemption was clear, at least by implication. Since the wrong ideas had debased them, the right ideas could save them. The solution to the "German problem" was re-education. Elementary schooling and higher learning, journalism and literature, the arts and sciences—all must join in molding a new mentality in Central Europe. Indeed, within ten or fifteen years after unconditional surrender, especially after the outbreak of the Cold War, American and British historians began to detect a heartening change in the outlook of the Germans. Hans Kohn, writing in 1960, even declared,

The German Federal Republic is part of the western community. The Germans have thereby given new strength to that community to which only a short while ago they presented a deadly threat. Germany's two hegemonial wars in the twentieth century brought the peoples and the civilization of Europe to the brink of catastrophe. Out of this catastrophe one great gain has come, a democratic Germany which is finally taking its due place in modern western life.¹⁴

There was hope for the redemption of Germany after all, but only through repentance and confession.

THE GERMAN HISTORIANS OF THE POSTWAR YEARS, however, were not contrite enough for most foreign critics. They were men who had reached maturity under the empire, who had lived through the First World War and the Weimar Republic, and who had witnessed the rise and fall of the Third Reich. They had not been fascists or racists, but neither had they been ardent democrats. Their political horizon was narrow, extending from cautious liberalism to moderate conservatism.

¹³ Mosse, *The Crisis of German Ideology: Intellectual Origins of the Third Reich* (New York, 1964), 1–2, 315–16.

¹⁴ Kohn, *The Mind of Germany*, 354.

Most of them had become *Vernunftsrepublikaner* during the 1920s; they had accepted the republican experiment *faute de mieux*, out of expediency rather than from inner conviction. After 1945, as their country lay about them in ruins, they had nothing to fall back on except the ideals of what had once been. They did not deny the tyranny or cruelty of the Third Reich, which they had just witnessed firsthand. But they maintained that 1933 represented a break rather than a fulfillment of the past. National Socialism was not a culmination of but a departure from the political spirit of Central Europe. There were other, more humane traditions that had once flourished on German soil and that must now be revived. In short, they looked back to the age of innocence their nation had known before the fall; they invoked that "other Germany" and its "good Germans," who had lived in righteousness until the First World War drove them down the slippery slope.

Friedrich Meinecke, the best known of the German historians of the pre-Nazi period, now more than eighty years old, urged his countrymen to return to the ideals of the Augustan age, to the spirit of Weimar. "The work of the Bismarck period has been destroyed through our own fault," he wrote, "and across its ruins we must seek the paths back to the time of Goethe." The great achievements of that age had been the creation of many men, united only by ties of friendship, who sought to realize the ideal of a personal and individual culture possessing at the same time a universal meaning and content. It was an ideal that could sustain the spirit of the nation in its present crisis. "We need no radical re-education to function effectively again as a member of the Western cultural community. Only the Nazi megalomania with its nonculture and its sham culture must be radically eliminated." That did not have to be replaced, however, by a pale, empty, abstract cosmopolitanism, but by a cosmopolitanism that had in the past been formed by highly individual achievements and that could be formed in that way again in the future. "The German spirit, we hope and believe, once it has found itself, will still have its special and irreplaceable mission to fulfill within the Western community."¹⁵ Only such a fulfillment could restore Germany to a respected place in the family of nations.

Franz Schnabel, a liberal Catholic from southwest Germany, author of a brilliant cultural and social history of his country in the nineteenth century, invoked the spirit of the German Confederation rather than the waning Holy Roman Empire. In the years after the Congress of Vienna there had still been an opportunity to check the disruptive nationality movements that romanticism had bred. "The demand for a compact national organism came from only a few parties; it was often a product of scholars." The improving technology of transportation and communication had in fact brought the nations of the Continent closer together, creating a world economy. "There was, therefore, a sound basis in the material conditions for a federal union of the peoples of Central and Eastern Europe." But Bismarck, by overthrowing the established order through militarism and annexationism, by achieving political unification under the domination of Prussia, "did the most to bring about the dissolution of Central and Eastern Europe into independent,

¹⁵ Meinecke, *Die deutsche Katastrophe: Betrachtungen und Erinnerungen* (Zurich, 1946), 168–69, 173.

purely national states." The minority nationalities, encouraged by the success of the German movement for political consolidation, took advantage of the weakness of the Habsburg monarchy. Once they became free, lying unprotected along the Russian border, Central Europe could no longer maintain its position. Bismarck had failed to recognize that the diplomacy of *raison d'état* would lead to a struggle for hegemony that had to end in the dismemberment of Europe. His triumph could be achieved only in the old way, "by violations of justice and ultimately by a decisive break with all historical relationships."¹⁶

To Hans Rothfels, however, Bismarck was the embodiment of the classic Prussian virtues of political responsibility and sober statecraft. An ardent German patriot, Rothfels had fought and been seriously wounded in the Battle of the Marne. But neither his service to his country nor the nationalistic tone of his writings and lectures at Königsberg could protect him against the Nazi insistence on racial purity. Forced to leave Germany on the eve of the Second World War, he was one of the few emigres to return after 1945. But while still at the University of Chicago, he published a study of the German resistance against the Third Reich in which he extolled the devotion to duty and patrician *noblesse oblige* associated with the old monarchical order. He denied that the conservatives who had opposed the Weimar Republic prepared the way for National Socialism:

It would be more appropriate to state that those traditions of a genuine "Prussian militarism" which still existed in Nazi Germany, formed a barrier against nationalist and demagogic excesses. . . . The leading military and aristocratic members of the conspiracy [against Hitler in 1944] certainly thought little in terms of a profession or a caste, and very much in terms of restoring human and supranational values. They were fundamentally driven by moral and religious impulses.¹⁷

To be sure, they had no intention of taking up where the ill-fated experiment in republicanism had left off. Their plans envisioned "a conservative and decentral-ized democracy with a more or less strong admixture of socialism." They were opposed to "mass democracy," but so were the founders of the American republic, Rothfels hastened to add, who also believed that unqualified majority rule must lead to tyranny. The ideals of the opposition to Hitler were in any case preferable to the "political setup of democracy," which the victors imposed on Germany after 1945, with its "party bureaucracy" and "large-scale democratic machinery" so alien to the historical experience of Central Europe.¹⁸

Some German scholars even argued that the ideological origins of the Third Reich should really be sought in Western Europe, in the concept of mass democracy and the doctrine of the sovereignty of the people. This was the position of Gerhard Ritter, for example, the most vigorous and prolific defender of his country's political tradition. Not only Germany succumbed to national self-idolatry, he contended. The fault could be found wherever Rousseau's *volonté générale* and the mythical "popular will" of the Jacobins were regarded as emanations of absolute reason and accepted as the highest court of justice. The search for the "roots of

¹⁶ Schnabel, "Das Problem Bismarck," *Hochland*, 42 (1949-50): 20, 23-24.

¹⁷ Rothfels, *The German Opposition to Hitler* (Hinsdale, Ill., 1948), 161.

¹⁸ *Ibid.*, 163.

National Socialism" would remain futile if its scope were restricted to Germany. For "not any event in German history but the great French Revolution undermined the firm foundation of Europe's political traditions. It also coined the new concepts and slogans with whose help the modern state of the *Volk* and the Führer justifies its existence."¹⁹

Ritter insisted that the democratic principle can offer no protection against a dictatorship. On the contrary, an egalitarian democracy is its most important political precondition. The freedom of a people who have become sovereign can turn into state slavery under a tyrant whenever some talented demagogue or brutal man of action succeeds in focusing on himself the trust of the masses with their yearning and their faith in the future, whenever he succeeds in making himself appear the bearer of the "popular will." We must recognize that "the system of 'totalitarian' dictatorship as such is not a specifically German phenomenon." It represents rather the radical shift from a democratic to an authoritarian organization of the state, which is possible wherever "the direct rule of the people derived from the 'revolt of the masses' is introduced."²⁰

It is tempting but unfair to dismiss Ritter as an unregenerate apologist for the conservative tradition in Central Europe, whose foundations were destroyed by the Second World War. He was more than that. He acknowledged that, while many states had bowed down before the idol of nationhood, "the Germans accepted all of that with special ardor when it was now preached to them by National Socialism, and their nationalism had in general displayed from its beginning a particularly intense, combative quality." Indeed, there was something poignant in the admission, made when he was in his seventies, that the convictions that had guided him throughout his life no longer seemed as sound as he had once believed. In the introduction to the second volume, covering the period 1890–1914, of his massive study of militarism in Germany, he wrote that he had looked back at those years "not without a sense of psychological shock." What he had portrayed there was "the prewar Germany of my own youth," which "has for an entire lifetime been illuminated in my memory by the radiant splendor of a sun that seemed to grow dark only after the outbreak of the war of 1914." But now, "in the evening of my life," the eye of the scholar could see "shadows that were much deeper than my generation—and certainly the generation of my academic teachers—was able to perceive at the time."²¹ It was a painful confession for a man who had dedicated his life to defending traditional civic values, only to discover in the end that they carried the seeds of corruption. Yet he and his generation found it emotionally and psychologically impossible to go beyond an admission that their views had perhaps been too uncritical and sanguine.

Even this admission was more than many of their countrymen were willing to concede. The dilemma of the German historians after the war was that, while they

¹⁹ Ritter, *Die Dämonie der Macht: Betrachtungen über Geschichte und Wesen des Machtproblems im politischen Denken der Neuzeit* (5th edn., Stuttgart, 1947), 186, and *Europa und die deutsche Frage: Betrachtungen über die geschichtliche Eigenart des deutschen Staatsdenkens* (Munich, 1948), 51.

²⁰ Ritter, *Europa und die deutsche Frage*, 51–52, 193–94.

²¹ Ritter, *Die Dämonie der Macht*, 186–87, and *Staatskunst und Kriegshandwerk: Das Problem des "Militarismus" in Deutschland*, 2 (Munich, 1960): 8.

were being criticized abroad for not enough contrition, they were being criticized at home for too much breast-beating. In his opening address to the first postwar meeting of German historians in 1949, Ritter warned,

We constantly run the risk not only of being condemned by the world as nationalists, but of actually being misused as expert witnesses by all those circles and tendencies that, in their impatient and blind nationalism, have shut their ears to the teachings of the most recent past. Never was our political responsibility greater, not only to Germany but also to Europe and the world. And yet never has our path been so dangerously narrow between Scylla and Charybdis as today.²²

German historical scholarship, having lost the ideological foundation that had sustained it for more than a century, had to find a new guiding principle appropriate to its changed circumstances.

ANY VISITOR TO GERMANY DURING THOSE YEARS who knew the language well enough for a casual conversation with a waiter, a taxicab driver, or a hotel clerk—or, for that matter, with a businessman, a public official, or a member of the learned professions—soon discovered what Ritter meant by the narrow path between Scylla and Charybdis. For the view that the Third Reich represented a perversion of the German spirit responsible for the collapse of the traditional political system of Europe was by no means accepted by all Germans. A common opinion, perhaps the prevalent one, was that, although the National Socialists had indeed committed acts of cruelty and oppression, the Allies were in no position to point an accusing finger. This assertion was generally followed by a long list of transgressions committed by the occupying armies—most of it, alas, only too accurate—ranging from murder and rape to black-marketeering, stealing watches, and assaulting civilians. *Die Anderen haben's nicht viel besser gemacht* was the constant refrain. The German historians were thus caught between those who criticized them for conceding too little, for not being remorseful enough, and those who criticized them for conceding too much, for not being defiant enough.

But during the 1960s the debate concerning Germany's guilt and redemption assumed a new form. The central issues remained the same, but the ways they were analyzed and evaluated changed significantly. Those who embarked on this re-examination were the young historians of a new generation, a generation whose formative experiences had been different from their fathers'. They had not been silent witnesses or unwitting accomplices of the Third Reich; they had been its dupes and victims. They remembered little about the Weimar Republic and nothing about the German empire, but they had very vivid recollections of the Hitler Youth, the Wehrmacht, the military hospitals, and the prisoner-of-war camps. Taught in their early years to serve loyally an authoritarian state, they had reached maturity in the bleakness of a divided country and a demoralized society. Unlike the older generation, they felt no responsibility, direct or indirect, for the

²² Ritter, "Gegenwärtige Lage und Zukunftsaufgaben deutscher Geschichtswissenschaft," *Historische Zeitschrift*, 170 (1950): 15–16.

rise of National Socialism. They felt no need to justify their behavior or explain their blindness to the dangers of totalitarianism. To them the Nazi regime represented an evil that had to be dissected and exposed, even if that meant condemning the political beliefs their fathers had embraced.

These younger historians, with no personal motives for defending the civic tradition of Central Europe, became its most bitter critics. They agreed with the German scholars of the postwar period that their country must find deliverance from the burden of guilt, but for them deliverance did not lie in a return to the spirit of the Augustan age, the German Confederation, or the German empire. It had to be sought in the future, in a new Germany, a democratic Germany, a Germany that had never existed but that had to be created. The new generation, like many foreign scholars after the war, argued that National Socialism was a logical culmination of the historical development of Central Europe. The great turning point had been 1945, not 1933; the end of the old German political tradition had come with the collapse, not the triumph, of the Third Reich. But, while foreign historians maintained that the corruption of Germany was caused by false doctrines, the young German scholars insisted that it resulted from the persistence of political and social institutions that refused to accept modernity, that fostered loyalties and beliefs incompatible with a democratic system of government. The task facing the Germans now was to bring about the rehabilitation of their country by a thoroughgoing reform of state and society.

The turn from the old to the new school of German historiography began in 1961 with the publication of Fritz Fischer's *Griff nach der Weltmacht*, which created a sensation in the world of scholarship. Fischer had started his academic career under the Third Reich as a student of religion and culture in Central Europe, but the experience of war, defeat, occupation, and reconstruction directed his attention to diplomacy, especially to the outbreak and course of the First World War. His massive study of the statecraft of that conflict emphasizes the readiness of the German government to use armed force in order to achieve world power. Based on a careful examination of a vast body of materials, published and unpublished, it reflects the author's view of the interrelationship between political actions and social interests. The foundations of German foreign policy, as he saw it, were shaped by ideological and material factors. The ambitions of the leaders of Central Europe, which in 1914 led to the outbreak of a world war, derived from a complex of attitudes and aspirations extending from Bismarck's empire to the Third Reich.

Fischer openly accepted the thesis of continuity in German history:

It would be too simple to make the emperor solely responsible for German policy before or Ludendorff during the First World War, as it has become usual to blame the Führer exclusively for the "fate of Germany" in the 1930s and 1940s. In fact, ideological motives based on religious and cultural traditions, on old or recent institutions and social structures, and certainly on material factors produce a web of forces in which the role of individuals can become significant, but only as one element among countless others that help condition it and make its effect possible.²³

²³ Fischer, *Griff nach der Weltmacht: Die Kriegszielpolitik des kaiserlichen Deutschland, 1914–18* (Düsseldorf, 1961), 11–12.

The bitter outcry against Fischer's work among the older German historians was not motivated primarily by differences concerning research or interpretation. That was only part of the story. More controversial was his implication that the responsibility of Germany for the outbreak of war in 1939 was not an isolated phenomenon reflecting the irrationality of totalitarianism but the manifestation of a historic pattern of recklessness and aggressiveness. This point was made even more explicit in Fischer's second major book, published in 1969, dealing with the years immediately preceding the First World War. Here he spoke of "the problem of continuity in the political and social development of the German empire from the late Bismarckian period on." To understand this continuity, we must recognize "the primacy of domestic policy" in determining the direction of diplomacy. According to the plans of the German statesmen, "a successful imperialistic foreign policy was to make the position of power of the ruling classes secure; they hoped actually to resolve the heightened social tensions through a war."²⁴ In the eyes of the older scholars, Fischer was giving support to the enemies of Germany; he was an ideological traitor. While they had been trying to defend their country by maintaining that the Third Reich was an aberration, he was providing ammunition to the other side by insisting that it was a continuity. Indeed, many of them heard in what he was saying an accusation of guilt directed against them personally.

For the younger German historians, however, Fischer's writings were a declaration of independence. They grasped the moral import of his work; they understood it as a summons to turn from the errors and delusions of the past. The generational conflict that ensued led to a historiographical revolution. This revolution involved first of all an abandonment of the classic themes in the writing of German history. The old school, committed to a traditional order, had dealt with subjects that reinforced the position of the political and social elites of Central Europe: the course of diplomacy and the conduct of war, the evolution of the state toward national unification, the rise of its dominant civic ideas and ideals, and the lives of the statesmen and soldiers who had made it great. The new school, ideologically on the left, turned to issues that would help democratize government and society: the structure of political parties and pressure groups, the process of industrialization and the rise of labor, the growth of population and urbanization, and the various defense mechanisms by which the ruling classes had sought to maintain their interests. The shift in the focus of historical scholarship that occurred in the course of only a decade was remarkable.

The change in the technique of research was as drastic as in its subject matter. That was to be expected, since methodology necessarily reflects ideology. The old school had relied primarily on the public or private records of prominent figures: documents, books, memoirs, letters, periodicals, newspapers, and proceedings. This material was summarized and presented in coherent narrative form, often with considerable literary skill, telling a story that the intelligent reading public could follow and enjoy. Yet the limitations of such an approach had become

²⁴ Fischer, *Krieg der Illusionen: Die deutsche Politik von 1911 bis 1914* (Düsseldorf, 1969), 12–13.

increasingly apparent. As early as 1949 Gerhard Ritter conceded that there was some justice in "the charge made by Americans today . . . that we have gradually become backward with our one-sided cultivation of political history in the narrow sense and our excessively sublimated cultural history, and that we have neglected to an astonishing degree the modern methods of the so-called social sciences." At least in the field of the most recent history, he continued, we find "a distinctly one-sided cultivation of diplomatic history, biography, and the history of ideas."²⁵

Twenty years later a group of young German historians began redressing the balance with a vengeance. They embraced the latest, most recondite techniques and concepts of historical research: quantification, demography, literacy, collective mentality, mass behavior, sociological theory, and social-science analysis. The outcome was a historiography methodologically more rigorous and ideologically more democratic, but so highly specialized as to lose the broad readership that the writing of history had attracted in the past. These changes in scholarship were not uniquely German; they affected the approach to historical learning in all countries. But in Germany they were pursued with special determination, because there more than elsewhere they represented symbolic atonement and vicarious regeneration.

Their effect was a basic shift in the historical perception of the modern development of Central Europe. That development came to be seen as the manifestation of a continuity of institutions and interests extending from the German empire to the Third Reich. The new generation analyzed in detail the entire bag of tricks and devices, in foreign as well as domestic policy, by which the established order maintained its control: "hegemonial wars" and "revolution from above," "organized capitalism" and the "interventionist state," "pseudoconstitutional semiabsolutism" and a "Bonapartistic dictatorial regime," "negative integration" and *Sammlungspolitik*, "social imperialism" and "religion as legitimating ideology." This was a drastically different approach to recent German history.

An impressive example was the work of Hans-Ulrich Wehler, one of the leaders of the new school, who maintained that the fundamental problem of Central Europe since the end of the eighteenth century was the "separate path" followed by the Germans. History must be understood as a "critical social science" that has to take into account the various "temporal structures" of a nation but that should above all encourage a "freer critical awareness of society." Such an awareness suggests that "the progressive economic modernization of German society ought to have been accompanied by a modernization of social relationships and of politics." But that was not what happened. The German empire was the only national state in the West that was established not only by means of a military "revolution from above" but also at a time when the agricultural revolution was ending and the Industrial Revolution spreading through Central Europe. As a result, the pernicious effect of the imperial power elites, reinforced by old traditions and new experiences, extended until 1945, "and in many respects even beyond that," in the form of "a penchant for authoritarian politics; a hostility toward democracy in the

²⁵ Ritter, "Gegenwärtige Lage und Zukunftsaufgaben," 9.

educational and party system; the influence of preindustrial leadership groups, values, and ideals; the tenacity of the German state ideology; the myth of the bureaucracy; the superimposition of caste tendencies and class distinctions; and the manipulation of political antisemitism."²⁶

TO BE SURE, THIS PICTURE OF THE GERMAN EMPIRE did not go unchallenged. The very methodicalness and logicity that make it so impressive have been criticized for rigidity and lack of realism. Otto Pflanze, after examining the techniques of Bismarck's statecraft, concluded that for some historians the models of "negative integration," *Sammlungspolitik*, "Bonapartism," and "social imperialism" have ceased to be merely heuristic tools and have become a fictive historical reality. Do such constructs have a more solid basis than concepts like *Volksgeist* or *Weltgeist* did in German idealism? he asked. We can draw them on paper, but do they exist in life? Thomas Nipperdey's objection went even farther. He contended that Wehler ignored the autonomous factors in the social and political structure, the divisions as well as the achievements of the established order, the social and political changes that were in the long run bound to affect the system itself, the temporary solutions and transitions that cannot be explained by a polarity between stabilization and democratization, and the relative stability of the empire as a whole. There must have been forces at work that made the Weimar Republic possible. A true history of Germany after 1871 can be written only in a comparative European and Atlantic context, he suggested. Only then, perhaps, will "our fixation on the struggle with our great-grandfathers" come to an end; only then will we be able to overcome the one-sidedness of today's historiography.²⁷

Yet most Germans have been incapable, for psychological rather than scholarly reasons, of undertaking such a comparative analysis. For them an acceptance of the uniqueness of the experience of Central Europe has served a pedagogical function essential to national rehabilitation. It was a confession of guilt, an act of contrition that must precede absolution. Foreign scholars, however, have felt no constraints of this sort. Their outrage at the crimes of National Socialism was gradually fading. There were now new conflicts, dangers, injustices, and villains. The history of Germany no longer seemed so drastically different from that of England, France, or the United States. In 1945 A. J. P. Taylor had insisted that the Third Reich was founded on terror, the secret police, and the concentration camp; it sought the enslavement of other nations. In 1961 he suggested that it was really quite similar to the Western democracies. Hitler and Mussolini may have glorified war and used the threat of war to promote their aims. "But this was not new. Statesmen had always done it." The rhetoric of the fascist leaders was no worse than the saber rattling of the old monarchs or, for that matter, "what English public-schoolboys were taught in Victorian days." Yet there had been long periods of peace before 1914 despite all the fiery talk. Even the dictators would not have gone to war unless

²⁶ Wehler, *Das deutsche Kaiserreich, 1871–1918* (Göttingen, 1973), 11–12, 17, 19, 238–39.

²⁷ Pflanze, "Bismarcks Herrschaftstechnik als Problem der gegenwärtigen Historiographie," *Historische Zeitschrift*, 234 (1982): 562–99; and Nipperdey, "Wehlers *Kaiserreich*: Eine kritische Auseinandersetzung," *Geschichte und Gesellschaft*, 1 (1975): 560.

they had seen a chance of winning. "The cause of war was therefore as much the blunders of others as the wickedness of the dictators themselves."²⁸

Geoffrey Barraclough agreed that the historical development of Central Europe resembled in many ways that of Western Europe, although to him the similarity lay in economic and social relations rather than diplomatic or military policies. "The unifying thread of modern German history," he wrote in 1972,

is seen to have been its response to the rise of modern industrial capitalism. In this respect its history is not basically different from that of other countries in other parts of the world, and one of the main consequences of the change of perspective is that it enables us to view the German past . . . "in the light of those coherent principles that may be applied to all political systems," instead of treating Germany as a pariah among nations, whose aberrations have to be condemned or explained away.²⁹

An even more sweeping exoneration came in 1978 from David Calleo, who portrayed the Germans not as aggressors or oppressors but as the victims of their neighbors' envy and sanctimoniousness. In a slim collection of essays, graceful rather than profound (originally published under the bland title *The German Problem Reconsidered* but translated into German more piquantly as *Legende und Wirklichkeit der deutschen Gefahr*), he presented his revisionist views with an urbanity that was captivating. "Imperial Germany was not uniquely aggressive, only uniquely inconvenient. Whatever faults and ambitions the Germans had were amply shared by the other major nations of the modern era." Unlike the British, Russians, or Americans, however, the Germans "lacked the space to work out their abundant vitality," and so, because of geography, that vitality became a threat to the rest of Europe. "Modern Germany was born encircled." Yet the Germans in fact resembled their neighbors, all of whom, as they became modern nation-states, displayed an aggressive, expansionist vigor. Like the United States, Germany underwent a late and rapid growth characterized above all by a talent for large-scale organization. Like the Americans, the Germans were latecomers to world politics, hence arrogant, insular, and ambitious. But, unlike the Americans, they lacked a continental backyard. "They also lacked the Anglo-Saxon talent for cant." Worst of all, they lacked the Americans' aptitude for hegemony and their training for imperial power. Since German society was more efficient and less exasperating than American society, it provided inadequate preparation for world management. Thus, the crises and defeats the Germans suffered in the twentieth century are a result not of their faults but of their circumstances.³⁰

The most systematic critique of the thesis of uniqueness and continuity in German history, however, came from a group of young British historians who in the late 1970s began to attack the work of the preceding generation, just as those historians had in the early 1960s attacked the work of their predecessors. Born after the Second World War, they had not experienced the events that colored the attitude of older scholars toward Central Europe. Though influenced ideologically

²⁸ Taylor, *The Course of German History*, 213, and *The Origins of the Second World War* (London, 1961), 103.

²⁹ Barraclough, "A New View of German History," 31.

³⁰ Calleo, *The German Problem Reconsidered: Germany and the World Order, 1870 to the Present* (Cambridge, 1978), 206–07.

by the left, like many of the historians of the previous twenty years, they reflected the views of the "new left," less rigid in its conceptualization, which wanted to examine history "from below." They sought, in the words of Richard J. Evans, one of their leading members, to emphasize "the importance of the grass roots of politics and the everyday life and experience of ordinary people." They maintained that the trend they represented "has taken place across a whole range of historical subjects, political opinions, and methodological approaches and has been expressed in many different ways," in the development, for example, of a "new school of 'people's history.'"³¹ These Young Turks of today regard yesterday's Young Turks as the priests of an outworn orthodoxy. The elaborate structure of concepts and analyses by which the latter had attempted to explain the fatal legacy of the German empire appears to them ready for demolition.

Thus in his study of the Center party in Württemberg, David Blackbourn has argued that, starting with the 1890s, public life in Central Europe was profoundly altered by the local railway, the small-town press, and the mobilization of the masses. "This process of political change was two-way: it operated upwards as well as downwards. On the one hand, an unprecedentedly large part of the population was attached 'from above' to a politics which already enjoyed an existence in parliamentary institutions and clubs; on the other hand, politics itself underwent a change as it acquired the imprint of popular aspirations," an imprint formed by "class and sectional interests," "the articulation of local identity," and "local grievances and aspirations." The demagogic manipulation of mass sentiment was by no means the exclusive preserve of the government or the conservative elite. It had a place in the repertoire of a much wider range of public figures, who were reacting politically "from above" to forces "from below." It would be misleading to view popular desires as having been simply taken up and acted upon by the politicians, but it would be equally misleading to construe political manipulation too mechanically, without taking into account the full force of pressure from the broad masses of the population.³²

The point that political life during the imperial era underwent a basic alteration that German scholars have ignored appears even more prominently in Geoff Eley's work on radical nationalism. "Somewhat against the grain of much Wilhelmine historiography," he has written, "I have conceived the post-Bismarckian period—influenced by processes of accelerated capitalist development, the end of the depression, and the passage to imperialism—as one of far-reaching political change, in which the entire structure of the public domain was re-ordered." His purpose has been "to liberate the analysis of the Wilhelmine right from the proto-Nazi teleology into which it has normally been co-opted" by examining the decomposition of the Bismarckian system after 1890 and the "complex social-historical process" by which the masses entered politics. This process, it became clear to him, had "transforming effects on the power bloc's hegemonic capability." The older historians, "imprisoned within [their] own model of Wilhelmine politics as a

³¹ Evans, "Introduction: Wilhelm II's Germany and the Historians," in Evans, ed., *Society and Politics in Wilhelmine Germany* (London, 1978), 22–23.

³² Blackbourn, *Class, Religion, and Local Politics in Wilhelmine Germany: The Centre Party in Württemberg before 1914* (New Haven, 1980), 18, 240–41.

manipulated system of institutionalized interventions *from above* which successfully 'socialized' the masses into the prevailing 'structures of domination,'" failed to recognize that "the process of popular mobilization seriously affected the forms and possibilities of ruling-class politics."³³

But the most exuberant, most defiant expression of Blackbourn's and Eley's revisionism may be found in a small book they wrote jointly on the "myths of German historiography." Here they have boldly attacked the "central thesis" of a "separate path" that the Germans allegedly followed because of the failure of bourgeois liberalism in their country. Such a view, Blackbourn and Eley have maintained, rests on "a curious mixture of idealistic analysis and vulgar materialism." They have rejected, moreover, the "exaggerated linear continuity between the nineteenth century and the 1930's," which only helped plant the seeds of National Socialism more deeply in German soil. It is simply not true that political life under the empire was stunted by the repressive intervention of the state. "Especially after 1890 there was a far-reaching trend to reinforce and sometimes even extend the freedom of assembly and association, the freedom of the press, and the rights of trade unions." Finally, they have asserted, it is a form of intellectual arrogance to judge conditions in Central Europe by the standards of Western Europe, to ask why Germany did not become England. Such an approach might once have served a polemical purpose; today it only shows the effect of the law of diminishing returns. It is *une question mal posée*.³⁴

These words have been music to the ears of countless Germans. Tired of the homilies and strictures of their own historians, they eagerly read what the foreigners had to say about the similarity between Central and Western Europe, between themselves and their neighbors. The historical judgments of Taylor and Calleo or Blackbourn and Eley sounded so comforting, so soothing. They seemed to whisper reassuringly: *Die Anderen haben's nicht viel besser gemacht*. They seemed to hold out absolution and redemption. Many German scholars, to be sure, regarded with bitterness the revisionist doctrines preached abroad. They had been trying for years, not always with success, to re-educate their countrymen, to convince them of their errors, to lead them back to the path of democratic righteousness. And here were the foreigners spoiling it all by telling the Germans that their sins were really only half sins or no sins at all. The conflict between supporters and opponents of the thesis of a "separate path" in Central Europe derived ultimately not from differences regarding scholarly research but from differences regarding the moral and didactic function of history.

THE CYCLE OF INDICTMENT AND APOLOGIA, GUILT AND REDEMPTION, however, may finally be coming to an end. It is now almost forty years since the end of the Second World War; most people living today have no recollection of that terrible conflict. What they seek is a sober, judicious understanding of what happened, without

³³ Eley, *Reshaping the German Right: Radical Nationalism and Political Change after Bismarck* (New Haven, 1980), 15, 355, 360.

³⁴ Blackbourn and Eley, *Mythen deutscher Geschichtsschreibung: Die gescheiterte bürgerliche Revolution* (Frankfurt am Main, 1980), 32, 56, 117, 124.

preachment or contrition. The recent appearance of works of historical scholarship written in this spirit suggests the beginning of a significant change in the historiography on Germany.

Lothar Gall's life of Bismarck, for example, the first important political biography by a German historian since the 1940s, reflects a new quest for dispassionate judgment. Neither deferential nor censorious, Gall has readily conceded that what the Iron Chancellor sought belongs to the past. But the means that he employed did have the effect, at the high point of his career, of accelerating the historical process. They helped bring about the emergence of what we call, in abbreviated form, the modern world. "Largely against his will he became at a decisive point the cocreator of that world."³⁵ Or consider the history of recent Germany by Gordon Craig, which has enjoyed even greater popularity among Germans than among Americans. To the author, what happened in Central Europe

demands the attention of reflective men, not only for what it has to teach about the role of fear and cupidity and obtuseness in human affairs, about the seductions of power and the consequences of political irresponsibility, and about the apparently limitless inhumanity that man is capable of inflicting upon his fellows, but because it also has much to say about courage and steadfastness, about devotion to the cause of liberty, and about resistance to the evils of tyranny.³⁶

Works such as these represent not only a more temperate approach to the past but also a return to a more traditional historical style and method. They emphasize clarity of narrative and gracefulness of expression; they seek to narrow the gap between professional scholarship and the intelligent reading public. More important, they foster a broad popular grasp of the interconnection between history and society. To a new generation of Germans, to whom the Third Reich appears neither admirable nor repulsive but incomprehensible, they may bring a deeper understanding of their country's past. And to a new generation of non-Germans, they may reveal how tenuous is the control of conscience over conduct, how fragile the foundation of decency on which a civilized community rests. For all of us, these are lessons worth learning.

³⁵ Gall, *Bismarck: Der weisse Revolutionär* (Frankfurt am Main, 1980), 729.

³⁶ Craig, *Germany, 1866–1945* (New York, 1978), viii–ix.

Reviews of Books

GENERAL

W. J. VAN DER DUSSEN. *History as a Science: The Philosophy of R. G. Collingwood*. (Martinus Nijhoff Philosophy Library, number 3.) Boston: Martinus Nijhoff; distributed by Kluwer Boston, Hingham, Mass. 1981. Pp. xiv, 480. \$47.00.

This book on R. G. Collingwood's philosophy of history began as a doctoral dissertation at the University of Leiden, but it goes well beyond any ordinary dissertation. W. J. van der Dussen spent a year in Canada, where he consulted with William Dray. He interviewed the late Sir Malcolm Knox, who edited two of Collingwood's posthumous books. The entire book was read in manuscript by Dray, W. H. Walsh, and Collingwood's daughter, Teresa Smith. Most important, the book makes extensive use of Collingwood's manuscripts and other unpublished writings.

The announced program of van der Dussen's book is to provide a systematic study of Collingwood's philosophy of history. Surprisingly, such a study (or, at least, such a study as the author actually gives us) has never before appeared in print. Most expositors have based their work entirely, or nearly exclusively, on Collingwood's *Idea of History* (and have thereby neglected most of what he wrote and published during his lifetime). In contrast, the present book tries to gather in virtually everything that Collingwood had to say about history not only in the *Idea of History* (most of which dates from 1935–36) but also in published and unpublished work done before and after those years.

The treatment is orderly. After a brief introduction, one chapter is devoted to the earlier published writings, another to the *Idea of History* and the subsequent critical discussion of it, a third to the unpublished manuscripts, and a fourth to Collingwood's studies as an archaeologist and historian of Roman Britain. These four chapters plus the introduction constitute the first two parts of the book (pp. 1–253). The final part (pp. 257–374) is, presumably, the systematic study intended by the au-

thor. This third part is, however, something of an anticlimax, for it repeats to a great extent points already made. It might have been better to have integrated much of this material into the two previous parts, leaving the genuinely new material intact and in better focus in a much-shortened third section.

What we do have, this defect aside, is a painstaking and compendious overview of R. G. Collingwood's work as a historian and a philosopher of history. Collingwood is by and large allowed to speak for himself. Van der Dussen quotes extensively from Collingwood's texts; this is especially welcome where the quotations are taken from unpublished sources. And readers of this journal will be especially interested, I believe, in a number of the things said, not least those in the chapter devoted to Collingwood's archaeological work. Here due weight is given to Collingwood's important studies of Hadrian's Wall, to his classification scheme for Roman brooches, and to his years-long epigraphy of Roman coins and other inscriptions, a labor undertaken, Collingwood says, as a "legacy from the late Professor Haverfield" (p. 436).

In the interests of brevity and of identifying further main points of significance, let me say simply that the principal value of van der Dussen's book lies in its use of Collingwood's unpublished writings. After Collingwood's death (his dates were 1889–1943), three books were published posthumously: two edited by T. M. Knox, *The Idea of Nature* (1945) and *The Idea of History* (1946), and *The Roman Inscriptions of Britain*, volume 1, *Inscriptions on Stone*, with R. P. Wright (1965). Collingwood had apparently authorized these publications (see p. 64 for the composition of *The Idea of History*), but his will declared him against any further publication of his unpublished manuscripts. For years the Collingwood family held these papers privately (about 3,000 pages in all); however, since March 1978 they have been available for consultation at the Bodleian Library, Oxford. (Selective quotation may be made with the permission of Teresa Smith, the daughter, but neither photocopying nor direct publication in

full is allowed.) In addition, the Bodleian has acquired manuscripts of some of Collingwood's notes and lectures on the ontological proof and on ethics (in the latter case spanning the years 1921–40); these have been available since May 1980 for consultation—but were not yet available to be consulted at the time the present book was written (see pp. xiv, 6–7, 127, and 395 nn. 1 and 2). Van der Dussen includes a composite list of manuscripts (of both the March 1978 and May 1980 sets) as an appendix to the book (pp. 445–54). A useful independent discussion of the unpublished writings (which highlights many of the features of the book and includes the listing) is found in van der Dussen's article, "Collingwood's Unpublished Manuscripts" (*History and Theory*, 18 [1979]: 287–315).

The unpublished material is of considerable intrinsic interest. It contains a number of Collingwood's lecture courses. A fragment of his unfinished magnum opus, *Principles of History* (1939), survives, the rest having been unfortunately lost (see pp. 64, 181, and 384 n. 19). There are several short papers on metaphysics and, in five "Woolworth" notebooks, a large (ca. 525 pages) treatise on cosmology dating from 1933–34. The manuscripts also give evidence of close attention paid by Collingwood to fairy tales, folklore, and primitive cultures, an interest not intimated by his published writings except for an occasional outcropping. And some new working papers of importance have been unearthed, most notably "Reality as History" (1935) and "Notes on History of Historiography and Philosophy of History" (1936).

Perhaps the single most important thing that emerges from this mass of unpublished material is a clearer idea of the development of Collingwood's theory of historical knowledge. Several commentators have expressed doubt about Collingwood's own account of the matter in his *Autobiography* (1939). But van der Dussen seems to think that these manuscripts support the *Autobiography*. I myself had come to believe, on the evidence of Collingwood's published writings, that an important sea change occurred in his theory somewhere between *Speculum Mentis* (1924) and *The Philosophy of History* (1930). Thus we find Collingwood saying in an article in 1925, "History assumes that there is a world of fact independent of the knowing mind. . . . [The historian] is always the spectator of a life in which he does not participate: he sees the world of fact as it were across a gulf. . . . The historian is thus always thinking of an object other than his own historical thinking . . ." (quoted from van der Dussen, p. 31). And this suggests a theory rather different from the one we know from the *Idea of History*. Drawing on unpublished notes and lectures on philosophy of history in the period 1926–28, van der Dussen is

able to date and describe this sea change much more precisely (see especially pp. 34–35).

Two important changes were involved, it seems. One, as outlined in van der Dussen, section 2.3, is Collingwood's switch from a realistic conception of the past (that is, the view that past events are *now*, in some sense, actual objects) to a conception of the "ideality" of the past (that is, the view that past events are finished, vanished, and gone and *now* no longer actual). The other is the emergence of Collingwood's solution to the problem of how historical knowledge is possible in circumstances where the historian cannot in any way perceive or be contemporaneous with past events, the objects of historical knowledge. This solution van der Dussen traces to the unpublished manuscript of Collingwood's lecture course on philosophy of history (written in France, at Die, in 1928), where the notion of the reenactment of past thought appears (see pp. 143–56, 309–10). It is interesting that Collingwood's first use of this idea concerned *music*, where he says that "the *sine qua non* of writing the history of past music is to have this past music *re-enacted in the present*" (pp. 144–45). A series of marginal notes in the lectures began to elaborate this idea to cover battles and then, ultimately, all past events. And in connection with these changes, van der Dussen brings out forcefully the strong Kantian flavor—and, presumably, Kantian orientation—of Collingwood's thinking about historical knowledge in these lectures.

Van der Dussen's book takes up an enormous number of interpretive issues regarding Collingwood's thoughts on history. The author exhibits an accurate knowledge of the vast secondary literature involved. He writes clearly and straightforwardly and manages to parry reasonably skillfully the thrusts and counterthrusts of some of the leading interpretations. His own judgment on many of the issues of substance is quite sound. For example, I liked his strong emphasis on the need to consider Collingwood's ideas about historical process; I thought his discussion of whether reenactment was a methodological notion and, in particular, whether it should be given an intuitionist interpretation was helpful and to the point. Occasionally, he does resort to refutation by quotation and even to citing out of context positions of which he is critical. But these faults should not obscure the virtues of the present book. Although I would be reluctant to describe it as a systematic philosophical treatise on Collingwood's theory of history, I would say that the book admirably serves a principal vocation: that of being a reliable handbook, almost an encyclopedia from A to Z, of what Collingwood has said about philosophy of history and what commentators have said about Collingwood. And in doing this job well, it will help set right the bigger picture, by helping us

avoid the vulgar errors and more common misunderstandings that have tended to cluster around Collingwood's philosophy of history.

This task is not unimportant. I would venture to say that by the end of the century Collingwood will be judged as one of the leading figures in British philosophy during the entire century. His philosophy of history, important to him as his chief project, will be regarded as his crowning achievement. In many ways Collingwood is the intellectual founder of analytical philosophy of history, and he remains one of its presiding geniuses. It is both timely and useful, then, that this very solid survey of Collingwood's writings on history and the philosophy of history should appear and itself contribute to our understanding at the point when Collingwood's unpublished writings are at last becoming available for scholarly scrutiny.

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ELISABETH YOUNG-BRUEHL. *Hannah Arendt: For Love of the World*. New Haven: Yale University Press. 1982. Pp. xxv, 563. \$25.00.

This first biography of the remarkable political philosopher Hannah Arendt (1906–75) obviously was a labor of love. Elisabeth Young-Bruehl's big, admiring book on her former teacher at the New School for Social Research is bursting with new information from sources of all kinds: manuscripts, letters, interviews, lecture notes, everything. Arendt's mother kept a record of baby Hannah's every new tooth and temper tantrum. Young-Bruehl continues the story almost in the same style. She is nothing if not mistress of the facts, but she does not always know what to do with them. Her tendency is not to interpret but to record—and to leave nothing out. She calls the result a "philosophical biography"; rather, it is mostly a chronicle: detailed, episodic, without sustained argument, and largely without conclusions.

A philosopher of the public life, Arendt remained secretive about her own private existence. Diligently, Young-Bruehl committed herself to uncovering the story. This genius on the nightmare history of our century, maintains Young-Bruehl, also had a "genius for friendship"; friends moved Arendt, sustained her; they were "the center of her life." Indeed, the author appears possessed of a particular fascination for those that Arendt and her second husband called "the tribe": her friends, his friends, their friends, friends from youth in Germany, more friends from a period of political exile in France between 1933 and 1940, and others from the years of Arendt's rise to intellectual fame in America.

They are all here. Some, in communications with the author, have entrusted her with their memories, letters, and the privileged details that fill her pages. Thus the intimate world of Hannah Arendt is revealed here with a knowledge that other biographers are unlikely to match, and Young-Bruehl must be admired for her careful reconstruction of the private events of a lifetime. One suspects, however, that private events are not what most readers will want to know about Hannah Arendt.

Mind you, narrative is not lacking on the public parade of her intellect. Recounted is Arendt's education in Germany, her shining presence among disciples of philosophers Martin Heidegger and Karl Jaspers, her work for Zionism, and, finally, her writing and teaching—to overflowing classrooms—in America. What remains clouded, however, are the things most important to understanding Arendt's creative and controversial brilliance: the deep structure of her thinking style, the source of her inspiration to elevate a particular perception of the "Jewish question" into a universal theory—at once metaphysical and concrete—of "World History," the origins of her dramatic concept of the twin identity of Nazi and Soviet totalitarianism, and the root emotions involved in her self-identity as a pariah. Wrote Arendt in her autobiographical biography of Rahel Varnhagen, "I am a Schlemihl and a Jewess" (p. 86).

Characteristically, Young-Bruehl wants to connect Arendt's ideas to private experiences, stressing the influence upon her of her husband, Heinrich Blücher, a former communist self-educated in the school of hard knocks. Their marriage resembled a permanent seminar. The book is least instructive on the genesis of Arendt's monumental *The Origins of Totalitarianism*, the work that made her reputation. Excellent, in contrast, is the chapter describing the storm over *Eichmann in Jerusalem*, when controversy invaded Arendt's friendships. Specialists know that Arendt did not always get the facts straight, but her interpretation of our world still holds power over the imagination. Young-Bruehl's readers will comprehend Arendt's hold on the affections of her friends and students, but they will not understand her magic appeal to those who have no private memories of her person. For them as well, reading Hannah Arendt has become part of a European education.

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MICHAEL E. HOBART. *Science and Religion in the Thought of Nicolas Malebranche*. Chapel Hill: University of North Carolina Press. 1982. Pp. x, 195.

Michael E. Hobart argues that the tension in Malebranche's mind between two "submerged models of thought," (p. 128) substance and number, fostered doubts about the compatibility of science and Catholic theology. Reflecting the mathematical character of the new science and challenging substance as a principle of traditional philosophy, the model of number reshaped Malebranche's epistemology. Because of the incompatibility of the new epistemology and the older but persisting ontology of substance, the understanding of such divine attributes as infinity and being became ambiguous, and a shift to hypothetical premises in logic denied even the existence of God a necessary affirmation. Such consequences were momentous, writes Hobart, and contributed to the anxiety of the Catholic soul in the seventeenth century.

At issue is Malebranche's Cartesian conception of science, a conception, according to Hobart, indifferent to sense experience and the actual scientific quest for nature's secrets. Moreover, the model of number has such broad implications in Hobart's study—providing a paradigm for a disjunctive sense of reality as well as the assumption of a correspondence between thought and the material world—that its association with even the mathematical usages that distinguished seventeenth-century science becomes tenuous. Consequently, Hobart's book adds only very indirectly to the persisting discussion of the relations between religion and the beginnings of modern science.

Hobart's methodological preoccupation with the implicit, though raising new questions, imposes a narrowness of its own on the scope of the problem even in Malebranche's thought. The intellectual historian's quest for the assumed but unspoken is potentially a profoundly enlightening enterprise, but do we assume, consequently, the superficiality of the explicit? Hobart states that Malebranche was explicitly committed to integrating Cartesian science into traditional Catholic Christianity, but we learn little about how Malebranche viewed this task, what problems he saw, and what strategies he chose. Nor, despite a passing reference to Malebranche's initial appreciation of the theological *advantages* of the new science and even the new epistemology, do we discover the final conclusions he reached. Hobart affirms Malebranche's adherence to the traditional natural theology but does not give due consideration to the possible significance of Malebranche's view that what pertains to science pertains equally to God or that our ideas of this world involve a union with the mind of God. The potential emotional import of such explicit and often emphatic themes is overlooked as Hobart concentrates on explicating theological difficulties that Malebranche never recognized.

The vicissitudes of the metaphysics of substance

and the rising philosophical influence of mathematics surely pertain to deeper shifts in thought that accompanied the emergence of a new science in the seventeenth century, and Hobart has provided a suggestive study that undertakes to explore those less accessible levels of conceptual change. He has not, however, fully developed the breadth and complexity of the relations between science and religion even in the mind of Malebranche.

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Boulder

MICHAEL LÖWY. *The Politics of Combined and Uneven Development: The Theory of Permanent Revolution*. London: Verso; distributed by Schocken, New York. 1981. Pp. 242. Cloth \$19.95, paper \$8.50.

STANLEY W. PAGE. *The Geopolitics of Leninism*. (East European Monographs, number 97.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press. 1982. Pp. 238. \$20.00.

Although on the face of it these books tackle similar topics, an exploration of the theoretical bases underpinning communist revolutions, the writers adopt sharply contrasting attitudes. While Stanley W. Page states categorically that Lenin's theory of world revolution did not die with him, because Stalin took it up after his death and implemented it in Eastern Europe at the end of the Second World War, Michael Löwy maintains that the continuation of Leninist ideology is to be found in Trotsky's works, and that Trotsky alone can claim the mantle of Leninism.

Page deals with the early controversies between Lenin and Kautsky on the validity of revolution in Western Europe and applies them to the little-known episode of the Bolshevik revolution in Latvia in 1917. The Latvians had been bolshevized because of the split between the indigenous proletariat and the German upper classes and were chosen to spearhead the revolution in Germany. Much of the argument is based on a typescript (apparently in the author's possession) by the commander of the Latvian Sharpshooters, Vatselis. Its validity could only be evaluated by inspection. Probably for this reason, the author neglects the creation of the Finnish Workers' Republic in January 1918, which was due to the same factors (though here the upper classes were Swedish rather than German) and which fought off the White forces until May, enabling the Russian Bolsheviks to consolidate. But the declaration on self-determination signed by Stalin in November 1917 applied to the Finns and the Ukrainians rather than the Latvians. Historians familiar with the Finnish Civil War may be justified in thinking that it was the Finns who were the "mid-

wives of the revolution," whereas the Latvians were merely the Bolsheviks' tools. Moreover, the Finnish brand of "national" communism has survived, while that of the Latvians has been absorbed into the Soviet mold.

Page concludes that, having failed in the West, Lenin turned to the East as the likely focus of the revolution. "Since then . . . the Lenin formula . . . has penetrated Latin America, the Middle East, Africa and Central and Southeast Asia. Lenin's formula for expansion is the major problem of those who oppose the rapid Communist advance" (p. 188). He also notes that American-style democracy has little chance of preventing socialist revolutions in Third World countries, as their poverty and backwardness make them an easy target for Marxism.

Such simple certainties are not available to Löwy. His arguments center on the theoretical differences of Lenin, Plekhanov, and Kautsky. He produces a painstaking analysis of the opposing concepts of "permanent revolution" and "socialism in one country" to demonstrate Trotsky's correctness and Stalin's error. His problem is the difficulty of providing a definitive answer in view of the vagueness of texts. Although Trotsky did say that a revolution was only conceivable as a dictatorship of the proletariat leading the peasantry, his formula is open to different interpretations: "Different countries will go through this process at different tempos. Backward countries may . . . arrive at the dictatorship of the proletariat sooner than advanced countries, but they will come later than the latter to socialism. A backward colonial . . . country, the proletariat of which is insufficiently prepared to unite the peasantry and take power, is thereby incapable of bringing the democratic revolution to its conclusion. Contrariwise, in a country where the proletariat has power in its hands . . . the subsequent fate of the dictatorship and socialism depends in the last analysis not only and not so much upon the national productive forces as upon the development of the international socialist revolution" (*The Permanent Revolution* [1970], pp. 279–80).

Noting, like Page, the fact that twentieth-century revolutions have succeeded in underdeveloped countries only, Löwy looks for theoretical support for this deviation from the classical theory. His conclusion is that "the unequal and combined development of capitalism in agriculture has produced a deep crisis in the rural life of the colonial . . . countries" (p. 211), and the theory has to be modified in view of this unforeseen phenomenon. "Because of the accelerated urbanization and industrialization of many peripheral capitalist countries, especially in Latin America, the revolutionary class struggles of the next decade may increasingly shift to the cities, and the working class will play a more

central role. The end of the twentieth century may see a return to the 'classical' October pattern of proletarian revolution" (p. 213).

Since both authors struggle with the problems of adaptation of theory to practice, albeit from very different viewpoints, it is worth pointing out that many Third World countries adopt the "socialist" model simply because it appears to be the quickest way to development. Such models are often supported by broad national coalitions of all classes. Therefore, each revolution is *sui generis* and depends much more on local conditions than on international factors. As Lenin discovered after 1917, pragmatism succeeds where dogmatism produces failure.

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WALTER CARLSNAES. *The Concept of Ideology and Political Analysis: A Critical Examination of Its Usage by Marx, Lenin, and Mannheim*. (Contributions in Philosophy, number 17.) Westport, Conn.: Greenwood Press. 1981. Pp. xii, 274. \$32.50.

This is a worthy and timely study of ideology as a rational and scientific method for critical examination of European philosophical and socioeconomic conditions as interpreted by Marx, revised by Lenin, and modified by Mannheim. It does not dwell upon ideology as myth or religion since none of the three social critics saw it that way. Walter Carlsnaes's study examines ideology as a relatively new approach that intermittently draws upon diverse sets of ideas, doctrines, opinions, values, theories, or philosophies to create a persuasive, ordered, and desirable new vision or societal model that should modify or replace the outworn, unworkable, existing socioeconomic order.

A brief introductory chapter defines ideology as a substantive and significant term. Carlsnaes examines various ideological, terminological, and conceptual disputes. His study is both critical in nature and "an attempt to pave the way for a methodologically acceptable and fruitful conceptualization of 'ideology'" (pp. 17–18). Three remaining chapters offer an explanation and critique of Marx's, Lenin's, and Mannheim's approaches to uses of ideology. An exhaustive survey of their definitions of ideology and an array of critical remarks by many scholars in this field offer a rich, often fresh and lucid view of this controversial subject.

In descriptive sections of his three chapters, the author examines Marx's philosophical conceptions of ideology, Lenin's reformulation and pragmatization of ideology as a source for an elitist party organization, and Mannheim's historical and sociological view of ideology. In critical sections of the

three chapters, Carlsnaes analyzes Marx's philosophical and sociological conceptions of ideology and Lenin's pragmatic, epistemological materialism and mobilizing conceptions of ideology. These are among the more interesting and informative parts of the book. A critique of Mannheim's substitute of the sociology of knowledge for "ideology" (where the thought is existentially determined) and his notion of utopia is presented with clarity. It brings into focus many differences between Marx's and Lenin's approaches to ideology. Mannheim's version of "utopia," according to Carlsnaes, is "perhaps his most un-Marxian idea" and "at the same time comes very close to Lenin's conception of mobilizing ideology" (pp. 220–21). Mannheim's contribution to the study of ideology is "of an almost Hegelian and dialectical development of the concept, a progression in which Marx can be considered as having provided the thesis, Lenin the antithesis, and Mannheim the synthesis" (p. 226).

Carlsnaes's well-documented discussion of the Marxian sociological conception of "ideology" as evaluative, descriptive, explanatory, and prescriptive is most helpful in unraveling the complexities of Marxian jargon. The greatest significance of the book, however, is its successful presentation of both the evaluative-critical aspects of Lenin's concept of ideology and his pragmatic use of ideology to organize and then execute a successful revolution. Lenin's use of ideology as a theory to seize power and, after the victory of revolution, as a guide to action, helps us to understand current revolutionary practices the world over. Carlsnaes depicts Lenin more as a tactician than as a theoretician of the revolution—as a totalitarian party organizer who manipulates ideology most aptly to bring to fruition his vision of the new world, regardless of the human sacrifices or destruction involved.

This book explores in depth some of the lesser-known aspects of Marx, Lenin, and Mannheim—the underpinnings for their theories that affected the fate and future of mankind. It is a welcome and useful reference study. One omitted topic that could be explored in a new book is the study of interplay between personal experiences and environment that contributed to the disillusionment and hostility of Marx and Lenin toward their respective political systems. A thorough critical study of the contrast between the utopia and the reality of Marxian and Leninist ideologies would create a more balanced view of the usefulness and limits of ideology as a tool for social change. It would eliminate possible future exaggerations of ideology as either a pivotal or an insignificant factor in societal change. It would discourage excessive or misleading declarations about "the omniscience" or "the end of ideology."

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RUSSELL JACOBY. *Dialectic of Defeat: Contours of Western Marxism*. New York: Cambridge University Press. 1981. Pp. x, 202. \$19.95.

For a couple of decades after World War II, speakers at the annual meetings of the American Historical Association regularly warned their listeners against the evils of "Marxism of the heart." They declared the mere condemnation of communist regimes insufficient: one had to abandon the illusion that it was possible (let alone necessary) to deal with Marxism impartially. These smugly tough-minded, liberal historians proscribed all philosophies of history that did not serve what they regarded as American political interests. They fed their students doses of R. N. Carew Hunt, G. D. H. Cole, George Lichtheim, and even J. Edgar Hoover.

The world has changed. Russell Jacoby declares at the beginning of his book that "the Russian and Chinese revolutions, imperialism, and the crises of capitalism all demonstrate the continuous validity of Marxism" (p. 11). Not everyone will agree with that, and one wonders who will accept the claim that the "strength of the working class" offers proof of the accuracy and success of Marxism.

Jacoby identifies the Soviet strain as "orthodox" Marxism, the standard against which to measure Western (or European) Marxism. He recognizes, however, that the Soviet variety has no existence outside slogans and incantations. What he does *not* see is the difficulty of getting one's bearings when testing the ideological purity of minority cults against the mumbo jumbo of establishment priests.

The author focuses on the European Marxists who lost their political struggles but won—in his view—the battle of the faith. Rosa Luxemburg, Georg Lukács, Antonio Gramsci, and Karl Korsch are central figures, as are the Dutch Marxists Hermann Gorter and Anton Pannekoek. Jacoby also discusses the Frankfurt School. Most of the heroes are representatives of the "Left Communist deviation"—a tendency Lenin hated because it refused to jettison the theory and exalt the party. Jacoby quotes Lukács to good effect here: "Organization is the form of mediation between theory and practice" (p. 61).

Jacoby deftly skewers Louis Althusser, arguably the silliest sixth-arrondissement Marxist of the last quarter-century. But he hedges in his assessment of Jean-Paul Sartre, whose political sagacity rivaled Oswald Mosley's but whose contribution to Marxist thought was substantial.

If anyone can sort out the Hegelian tradition, it is Jacoby. He rightly calls attention to the antagonistic interpretations of Hegel that have long separated Soviet and European Marxists. The linking of an explication of Hegel's legacy, however, with a vigorous assault upon Engels (and this in a chapter entitled "The Marxism of Hegel and Engels"), adds

perhaps a gram too much confusion. But it is just this juxtaposition of penetrating analysis and provocative—if occasionally outrageous—judgment that makes this book so refreshing and stimulating. Jacoby has written one of the most challenging books on Marxism ever to come from the pen of an American author.

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JOHN MOLYNEUX. *Leon Trotsky's Theory of Revolution*. New York: St. Martin's Press. 1981. Pp. xii, 252.

It is courageous to attempt a critical evaluation of Trotsky's thought, as John Molyneux does, without knowing the Russian language or consulting all the available works of scholarship in non-Russian languages. And it is bold to approach Trotsky's ideas without attempting objectivity. This account is frankly partisan. As the author admits, it derives from the theoretical tradition of the British Socialist Workers' party. For all of the above reasons the book will not satisfy most scholars, though the author has written a lively, brisk, and intelligent essay on Trotskyism then and now.

Scholars will wish that Molyneux had referred more frequently to the excellent study of Trotsky's political thought by B. Knei-Paz, or that he had consulted R. B. Day's evaluation of Trotsky's economic thought. Knei-Paz is accused of being "mistaken at every point," and Day receives a single passing footnote. Isaac Deutscher is quoted approvingly, but more recent works by I. Howe or J. Carmichael are not mentioned at all, and neither are the other assorted works in non-Russian languages. The Trotsky archives in the Houghton Library at Harvard University were not consulted at all.

The value of Molyneux's highly subjective and personal interpretation is its refreshing directness. Like many others, Molyneux pictures Trotsky as a revolutionary theorist who was frequently out of touch with the reality of his own time. Trotsky was also guilty of ignoring the need for creating political and organizational agencies that would bring about the goals of Bolshevism. He left behind no "system" or "program" of international revolution. But, unlike most observers, Molyneux believes that Trotsky had a more profound insight into the great rhythms of history than most Marxists of the twentieth century. For one thing, Molyneux points out, Trotsky correctly perceived that revolution was inevitable in Russia in 1917 and that there were only two possible outcomes, "dictatorship of the proletariat or the dictatorship of Kornilov." Unlike most of his contemporaries in the social democratic movement, Trotsky believed that to bring about the "inevitable" socialist revolution, even in economically advanced

countries, would require human intervention and initiative.

Molyneux's major thesis is that Trotsky saw more clearly than his contemporaries that for a Marxist revolution to be successful in terms of its avowed goals it must be made by workers, not peasants or bureaucrats. Molyneux indicts, therefore, most of the Marxist revolutions of the twentieth century, which have brought one-third of the population of the world under their sway. Yet there is some merit to his conclusion that the shortcomings of both the Chinese and the Russian revolutions, especially their authoritarian statism, grow out of their domination by parties that do not have a constituency.

Students of international revolution will find this book interesting and provocative, providing they understand that it is more of a polemical essay than a scholarly analysis.

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WILLIAM G. ROSENBERG and MARILYN B. YOUNG. *Transforming Russia and China: Revolutionary Struggle in the Twentieth Century*. New York: Oxford University Press. 1982. Pp. xix, 397. \$19.95.

This engaging work juxtaposes the Russian and Chinese revolutions from the beginning of the twentieth century to the 1970s. William G. Rosenberg and Marilyn B. Young draw on published historical writings, memoirs, literary works, and political documents to synthesize a vivid account of both revolutionary histories and to explore a number of common interpretive themes. Early chapters of the book recount the emergence of revolutionary situations and communist movements, focusing separately on Russia (chaps. 1 and 2) and on China (chaps. 3 and 4). Once the narrative reaches the 1930s, however, every subsequent chapter ranges back and forth to cover simultaneous sets of events in both countries, moving from "Party-Mass Relations in the 1930s" through the "Great Patriotic Struggles" of World War II, from there to "Socialist Roads" and "Great Leaps Forward" in the 1950s and 1960s, and finally to the periods of "revisionist" stabilization in the Soviet Union after the death of Nikita Khrushchev and in China after the passing of Mao Tse-tung.

In any work of comparative history, choices about subject matter and time periods to be covered and compared naturally express the authors' special understanding of their subject matter. Here Rosenberg and Young most fundamentally cover each revolution in its own terms as a lengthy *process* viewed "from the inside out" (p. x)—that is, from the perspective of the articulate and self-conscious leaders who pursued change from the time of the Old Regime through the struggle for revolutionary

state power to the final sputterings of revolutionary impulses some decades thereafter. The authors are wonderfully sensitive to the dilemmas faced by the Russian and Chinese revolutionaries as they grappled with the challenges and recalcitrant limits of their respective societal contexts. And they dramatically highlight the courses of action facilitated by developing Bolshevik and Chinese communist ideologies—especially the bureaucratic conquest of the “backward” peasantry in Soviet Russia, in contrast to the political mobilization of the peasants as makers of revolution in China.

Rosenberg and Young chose to juxtapose their two accounts in strictly chronological parallel chunks. This approach becomes clumsy and confusing at times, and it stands in the way of rigorous comparisons (of Old Regimes, new regimes, or analytically similar phases of the revolutionary conflicts). Nevertheless, some interesting and unusual angles of vision result. For example, the stretching of the Russian Revolution into the 1960s leads to a view of Khrushchev’s regime in Russia as a final effort at revolutionary revitalization, with some intriguing parallels to the Maoist “Cultural Revolution” in China. Moreover, the parallel effect of World War II in fusing revolutionary legitimation with popular nationalism is dramatically underlined. Finally, too, by emphasizing simultaneous developments, Rosenberg and Young are able to pay steady attention to the lessons that Russian and Chinese communists are learning from the other country’s experiences. The result, indeed, as the authors intend, is a richer sense of actually unfolding historical processes involving actors who make conscious choices.

Readers thoroughly familiar with the relevant specialist literatures will find little strictly new material in *Transforming Russia and China*. But the book’s perspective is fresh and its historical treatments nuanced and intelligent. For some time to come it will be rewarding reading for nonspecialist historians, students, and members of the educated public who want a sympathetic introduction to strategies and struggles in the two greatest social revolutions of our century.

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DAVID VITAL. *Zionism: The Formative Years*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xviii, 514. \$29.95.

A good historian steps out of his own shoes and tries to see conditions in light of the time about which he writes. Now that the state of Israel is well established and militarily powerful, it is imperative for the

historian of Zionism to depict clearly the small size, debilitating internal divisions, and other weaknesses of Zionism at its beginning. David Vital has done this with surpassing intelligence, great felicity of expression, and excellent control of the available Hebrew, German, Turkish, Russian, English, and Yiddish primary sources and secondary literature.

Zionism: The Formative Years covers the period beginning with the reshaping and revitalization of the Zionist movement in 1897 (the occasion of the first Zionist Congress, held in Basel) and ending with the all-Russian Zionist conference in Helsingfors (1906). Vital traces internal Zionist history against the background of Russian, West European, Ottoman, and Palestinian diplomatic, economic, and social developments. Zionist detail is always related to historical context. Consequently, the overwhelming odds against Zionist success clearly emerge. Among other factors, Ottoman hostility, German-Jewish opposition, Orthodox Jewish suspicion, Wilhelmine vacillation, and much indifference on the part of Jews who preferred to leave Russia for the West and of gentiles who saw the Jewish problem as neither demanding nor irksome, combined to render the early Zionists a tiny minority (a mere 1 percent of Jewry were dues-paying members of the Zionist Organization in 1898) and, in the eyes of many, a confederation of eccentrics.

Vital shows how the personality and program of Herzl skillfully breathed new life into the East European *Hibbat Zion* proto-Zionist societies, thereby giving him a base to attract the attention, if not the cooperation, of the great powers. Herzl’s diplomacy, in turn, even while it was seen as fruitless and distracting by much of Russian Jewry (the real pool of potential Zionists), tapped their energy and commitment. Thus, when Herzl’s diplomatic efforts to win European or Ottoman sponsorship of a Jewish entity failed, the movement that he had built to make his diplomacy credible sustained itself. The varying fortunes of what were called political, practical, spiritual, and religious Zionism are here accurately and lucidly portrayed.

This book is the result of prodigious scholarship, though that scholarship is inadequately reflected in the thin notes, and those notes are not collated in a bibliography (there is an essay on bibliography). Thus nonspecialists will learn much about Zionism but not enough about the sources on which this book is solidly constructed. The book’s superb readability, wide contextual perspectives, and fine organization make it most suitable for classroom use. *Zionism: The Formative Years* continues Vital’s *The Origins of Zionism* (1975). One can only look forward to the volumes to come. They promise to constitute a definitive history of Zionism.

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ROBERT GRIFFETH and CAROL G. THOMAS, editors. *The City-State in Five Cultures*. Santa Barbara, Calif.: ABC-Clio. 1981. Pp. xx, 237. \$22.50.

The genesis of this book is put forward in the preface with a certain charm, since it appears as parallel to the ancient Broadway theme: "Hey, kids, let's put on a show!" In this more serious case, a group of scholars teaching in approximately the same geographic area have gathered to attack the problem of the essential nature of the city-state—certainly a valid historical enterprise.

Of the five cultural areas chosen for the city-state phenomenon that appeared within them, four are no surprise: ancient Sumer (examined by Song Nai Rhee), Greece (Carol G. Thomas), northern Italy (Gordon Griffiths), and the German and Swiss zone (C. R. Friedrichs). The fifth, the Hausa society of sub-Saharan Africa dealt with by Robert Griffeth, is a welcome addition, not least because its inclusion shows that data from the immense and varied African area are increasingly available for analysis.

Griffeth's contribution is, in fact, rather more sharply in focus and convincing in conclusion than those of some of his colleagues. Rhee's look at Sumer is marked by the peculiar view of the archaeologist—I use "peculiar" here following Lewis Mumford, who cheerfully jumped on his archaeological confreres for a mental set dominated by *strata*. I think that Rhee is in error to follow Thorkild Jacobsen in deducing a "democratic" genesis for these city-states; I think he is also in error to identify the Sumerian *abba uru* ("town fathers") as "heads of . . . large families" (p. 17): he must mean *clans*, which describes a different social and political picture.

Thomas's study of the Greek polis ought to be central to the structure of this book, but it is overall the least satisfactory contribution. Admittedly Thomas is batting in a tough league, marked by scholarship both elegant and percipient: her views too often show neither quality. Awkward or misleading phrases or usages tend to block whatever meaning she might be leading toward. "Officials," for example, is used for every variation in city-state leadership from party leaders to generals to true administrators. She seems to regard Sparta as a monarchy (p. 63), gives the "role of avengers" to the Athenian Areopagus (p. 58), and has a very cloudy notion of the role of religion in the polis: the list could be much extended.

But a collection of studies need not, like a chain, be seen as only as strong—or convincing—as its weakest link. Griffiths's examination of the medieval Italian city-states is sharp, informative, and analytically satisfying. If the study is skewed toward Florence this is understandable if regrettable, for Florence, like Athens, seems to have been a mutation.

Friedrichs's description of the German and Swiss city-state formations is solid if occasionally repetitive, and like Griffiths on northern Italy he is perceptive in showing the complex social, political, and economic permutations running through the *Freistädte*.

After Griffeth's admirable excursus on the Hausa city-states, with their notion of citizenship defined as kinship and their highly developed ability to absorb extraneous elements, he and Thomas attempt to formulate a conclusion. A "basic model" of the city-state is not constructable, according to their data and analytic approaches: so much would already be clear to the reader. Almost every possible variation in political mode, economic structure, and social differentiation may appear in the type, and a list of *common* characteristics is not very informative. Could another attempt at defining the city-state in history do better than has been done here? Probably not—not without a certain amount of provocative risk taking or hubris in hypothesizing. This collection, then, has its value as a compromise solution and, in the main, a careful cross-cultural sampling of city-state forms.

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ROBERT B. EKLUND, JR. and ROBERT D. TOLLISON. *Mercantilism as a Rent-Seeking Society: Economic Regulation in Historical Perspective*. (Texas A&M University Economics Series, number 5.) College Station: Texas A&M University Press. 1981. Pp. xiii, 169. \$17.50.

IMMANUEL WALLERSTEIN. *The Modern World-System. Volume 2, Mercantilism and the Consolidation of the European World-Economy, 1600–1750*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xi, 370. \$22.00.

These volumes are part of a growing literature concerned with exploring the historical roots of present-day Western economic predominance. Both focus on the mercantilist period of Western history; both extend their analysis from the nation to the world; both provide a mechanistic, neo-Marxist perspective of exploiting nations at the Western "core" and exploited nations or areas at the "periphery"; both are the work of nonprofessional historians (Immanuel Wallerstein is a sociologist, Robert B. Ekelund, Jr. and Robert D. Tollison are economists). The books differ in scope and organizing principle. Wallerstein's historical analysis, which follows in the tradition of Max Weber and Fernand Braudel, is the second of a projected four-volume work aimed at providing a coherent understanding of the economic and social transformation of capi-

talist Europe since the sixteenth century and its impact in the world. More modestly, Ekelund and Tollison provide a reassessment of mercantilism in the light of *positive* economic theory, which stresses the rational pursuit of state-sanctioned monopoly rights defined as "Rent." Wallerstein's organizing principle is his "World-System"; that of Ekelund and Tollison is "Rent."

Wallerstein begins (in chap. 1) by analyzing the economic process that he believes prevailed throughout the period 1600–1750. Chapter 2 considers "Dutch Hegemony in the World-Economy" prior to 1650; chapter 3, "Struggle in the Core-Phase I: 1651–1689," explains how England and France came to unseat the Netherlands in the contest for world supremacy; chapter 4 discusses "Peripheries in an Era of Slow Growth" (Eastern Europe excluding Russia, Southern Italy, and parts of Latin America); and chapter 5, "Semi-peripheries at the Crossroads" (Spain, Portugal, Central Europe, Scandinavia, Russia, and British North America), shows how and in what circumstances "core" and "peripheries" interacted. The concluding chapter (chap. 6) gives a fairly standard account of the ongoing "Struggle in the Core-Phase II: 1689–1763" between the English and the French from which the English emerged triumphant. There follow sixty pages of bibliography.

The basic premises of Wallerstein's second volume are similar to those of the first. In explaining the dynamics of the seventeenth and eighteenth centuries, emphasis is again placed upon the external economic environment. As before, the mass predominates; the "core-periphery" paradigm remains unchanged, linkages are economic and capitalistic; "semi-periphery" states continue to shift from one status to another; "free" labor is in the "core" areas, "coerced" labor in the "peripheries"; the strong still exploit the weak. There is the same countless mass of footnotes ready to enlighten and sometimes enmesh. Wallerstein's second volume differs not in approach but in the context in which his theory is applied. In contrast to the long, expansive inflation of the sixteenth century (the author's earlier Phase A), in which his "World-System" took root, the period 1600–1750 (Phase B) was a time of relative contraction and depression. Contrary to the accepted treatment of the seventeenth century as a time of economic troubles, Wallerstein maintains that these years were a crucial stage in the development of the next phase of world capitalism.

Although the work of Ekelund and Tollison covers much of the same ground, it is more succinct and theoretical. With "Rent" as the mono-cause, chapter 1 portrays "Mercantilism as a Rent-Seeking Society"; chapter 2 offers "The Rent-Seeking [economic] Model," which, it is claimed, "is more useful in explaining developments in the mercantile econo-

mies than the usual specie-accumulation interpretation" (p. 28); chapter 3, "Economic Regulation and Rent Seeking in Mercantile England," and chapter 4, "The Venal Society of Mercantile France," compare the experience of the leading mercantile states; chapter 5, "Industrial Organization in the Mercantile Age," emphasizes the monopolistic elements of the early trading companies and also traces the relation between the appearance of the business corporation as "an actual and conceptual entity" and mercantilism. Chapter 6 provides a "Summation and Conclusion." There is an adequate bibliography.

To the extent that both books are scholarly and challenging, they demand respect. In continuing to elaborate his thought-provoking, schematic concept of a new "World-System," Wallerstein demonstrates his prodigious learning. Although the contribution of Ekelund and Tollison is less comprehensive and less visionary, it does at least—without trying to refute the pioneering work of Eli Heckscher or Jacob Viner—cast new light on the crucial link between "Rent" or monopoly profits and institutional change. No one can read either book without being better informed about the role played by mercantilism in the origins of the modern world.

Any assessment of such wide-ranging studies of world development must depend to some degree on the *Weltanschauung* one brings to bear. This kind of inquiry certainly cannot be dismissed by quibbling over detail. For those who are already convinced that profit-maximizing resource allocation played the overwhelmingly paramount role that these writers aver, there is little left to argue about. The perception of possible gain *might* have resulted in the creation of a "World-System" with the economic categorization described here, but did it? The vagaries of modern economic development *might* have been due to the ongoing, changing economic relations between "core" and "periphery" (in which the contribution of the "periphery" to the "core" appears exaggerated), but were they? "Rent" could be the key to mercantile developments, but was it? The evidence given here—taken by itself—does not allow us to form firm conclusions either way. The fault is in trying to simplify an extremely complex situation—one that is too big and too imprecise—by placing stress on a single factor. Religion, politics, and noneconomically oriented creative minorities must surely have played a more influential role than they are credited with. The emergence of the modern world might not only be explained by the development of capitalism but also by the appearance of a system of politically competing states; changing levels of organized force in the world might be just as important as changing levels of fortune. Perhaps one day we shall be able to devise an all-encompassing scientific theory or system that

unambiguously explains how the modern world came to be what it is. But that day has not yet come. The merit of these volumes is not that they provide final answers but that they assist in further investigations, which is, after all, as much as any sensible author could hope for.

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ALAN WATSON. *The Making of the Civil Law*. Cambridge: Harvard University Press. 1981. Pp. xii, 201. \$22.50.

From the title one might conclude that this book by Alan Watson is a brief introduction to the creation of the Corpus Juris Civilis. In fact, however, it is a discussion of the difference between legal systems based on the English common-law tradition and those based on the Roman-law tradition, with the aim of explaining why the civil-law tradition functions as it does. The conclusion offers nine factors that effect changes in a legal system.

To explain the differences between common-law and civil-law systems in the modern world by saying that the latter legal system is a codified one but the former system is not, Watson argues, does not explain anything. Why do some societies codify their laws while others do not? After considering a number of reasons for codification, Watson concludes that the fundamental reason for the codification of a society's legal system is the existence of a long tradition of Roman-law studies. In contrast to those who see law purely as the product of political, economic, and social pressures within a society, Watson sees a codified legal system as the product of an intellectual and educational process that trained lawyers to want an orderly legal system, a code that reduced the law to a set of fundamental rules. This desire was not related to contemporary societal needs and interests. In addition, Watson argues that eighteenth-century natural-law thinking and the Enlightenment interest in rationality and order played a less significant role in the creation of modern codes, even Napoleon's, than did the existence of a tradition of Roman-law studies. The proposals for achieving changes in civil-law systems are based on this analysis of the intellectual roots of the codification process.

Watson's argument is an interesting one because it attacks some currently fashionable theories of the relationship between law and society. In this short book, however, he can only touch briefly on any number of topics that are relevant to his argument. If one has read the author's other books on Roman law and accepts his general approach, then this book would probably appear convincing. To a reader without that background and point of view, Wat-

son's book is an argument for which some illustrative materials have been provided, but which lacks an adequate history of the development of Roman-law studies. Under those circumstances the reader might not be convinced by the argument.

In addition to providing only a sketchy historical basis for the argument, Watson has provided only relatively few notes and has not provided any bibliography. The result is a work that should interest those concerned with how legal codification has proceeded in the modern world and some of the implications of that process for the work of reforming the law. Legal historians, however, will probably find themselves asking a number of questions about Watson's argument and conclusions.

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OLIVE BANKS. *Faces of Feminism: A Study of Feminism as a Social Movement*. New York: St. Martin's Press. 1981. Pp. 285. \$25.00.

Historians over this century have written about the varieties, diversities, and discontinuities in feminist thought and collective action, usually emphasizing that modern feminist movements have run parallel and related courses but in general have been separate entities to be studied in terms of time, place, culture, and circumstances. Now, in *Faces of Feminism*, Olive Banks surveys nineteenth- and twentieth-century feminist thought and organization in Britain and the United States in an analytical and comparative framework that allows her to write of *the* British feminist movement and *the* American feminist movement. She "attempts to see the 'old' feminism and 'new' feminism as one single historical process and . . . how one movement developed into the other" (p. 1). The unifying factors that permit her a holistic approach and perspective consist of her broad definition of "feminist" and organized feminism as "any groups that have tried to change the position of women, or the ideas about women and the great variety of ways in which they have tried to do this" (p. 3); her perspective from a great time-span of the early nineteenth century to the present; and her major contextual stance of viewing organized feminism from "three intellectual traditions, originating in the eighteenth century but continuing to operate as differentiating principles even to this day." The three traditions that Banks identifies are those of Evangelical Christianity with its emphasis on concern for social issues and public action; the Enlightenment with its propagation of principles of individualism, natural and equal rights, and freedom from authority; and Socialism as it derives from early communitarian socialism and the

Saint Simonian movement in France. These common roots, says Banks, do not lose their identity but unify the feminist movements of past and present, while the different emphases and values among these same traditions account for the different "faces."

Banks's survey of both the British and the American feminist movements is based on recent first-rate secondary sources that are credited parenthetically in the text. After an introduction that justifies the framework and expresses the goal of reaching tentative explanations and raising questions, the chapters follow in chronological order: "The Early Years, 1840-70"; "The Golden Years, 1870-1920"; "Intermission, 1920-60"; and "The Modern Movement." Banks shows the relationships and alliances between feminist and other social movements including antislavery, social welfare, rights of labor, birth control, social purity, suffrage, and "new woman" activism. The discussion proves the author's wide acquaintance with feminist ideas, goals, and pressure groups of both nations as well as with the best known among the leaders. (Individuals like Annie Besant are exempt because they were not identified with an *organized* movement.)

While the survey covers ground already familiar to historians, the factual content, the comprehensive discussion, and the scholarly stance with analytical framework combine to make this a valuable text for students and a very useful one for knowledgeable teachers. For the history scholar, Banks succeeds in her goal of raising questions about the breadth of the context, the qualitative nature of the comparative methodology, the generalizations concerning feminist continuity and diversity, and the conclusions about work still to be done if we elect to place high priority on attempting to evaluate organized feminism as a "general phenomenon." *Faces of Feminism* gives historians a good basis for reviewing the goals and assumptions of scholarly women's history.

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HOWARD LAMAR and LEONARD THOMPSON, editors.
The Frontier in History: North America and Southern Africa Compared. New Haven, Conn.: Yale University Press. 1981. Pp. xii, 360. Cloth \$37.50, paper \$7.95.

The comparative method can illumine the past of a single society, can shed light on two or more historical cases, or can provide the basis for synthesis and grand generalization. *The Frontier in History*, edited by Howard Lamar and Leonard Thompson, is a thoughtful and well-constructed example of the second type. New ways of looking at the less familiar

and less researched case of South Africa emerge by being contrasted with what is known about the expanding American frontier.

Hermann Giliomee's well-handled essay in this volume shows with exactitude how the South African experience differed from that of the United States. Implicitly, he helps answer the fundamental questions: Why did South Africa evolve into an intolerant, apartheid-dominated society when the United States did not? To what extent may the experience of the frontier have conditioned varied responses?

A key variable is size, another is resources, and a third is technology. There is a fourth, too, which may have been more important than the others, but Giliomee focuses on the first three. In terms of the triad, the Americans were blessed (as other chapters in this volume show) with a wide expanse into which successive waves of pioneers could expand. There was abundant rainfall, too, which South Africa lacked.

Because each wave of frontiersmen was connected socially and economically (and in terms of transportation and communication) to a thriving and expanding hinterland, the American pioneer was part of what Giliomee aptly calls a capitalist revolution and an "industrial ferment" (p. 118). Although his South African compatriot trekked away from authority to subsist, the American frontiersman supplied distant and not so distant markets, rarely seeking to escape entirely from the original colonial network from which he and his fellows had come.

South Africa was smaller in extent until the 1870s and 1880s, was linked far less directly than was the U.S. to the engines of European capitalism, and was much more impoverished. Good land and wild game were scarcer and more rapidly exhausted than in the American West. Access to capital was severely limited compared with the United States. South Africa's only sizable town was too small and too distant to provide a suitable market for trekkers. Local markets, other towns, and communications were slow to develop. Nor was there an overseas market for South African primary products until diamonds and then gold were discovered toward the end of the nineteenth century. The trekkers, too, were Afrikaners, and were largely antagonistic first to the Dutch East India Company and later to colonial rule by Britain. Loyalty to the metropole or the national government (which was in Europe, not America) never developed. Nor did the frontier provide the kind of unity that was experienced in America.

An even more formative difference, as the editors recognize, was the nature and number of the inhabitants of the frontier lands. Although the melting-pot Americans met some native Americans who were settled agriculturalists, many were not. The

native Americans were thinly settled and competitive with the frontiersmen only in selected areas. In South Africa the indigenous population competed for land and game throughout the eighteenth and nineteenth centuries. On the eastern Cape Colony frontier, in particular, settled hoe-agriculturalists constituted a major, populous obstacle. The Afrikaner trekkers were inferior in quantity in a way that the Americans were not. North America had smallpox, too; it has made a major difference.

For reasons of number and competition, but also because of the prior experience of slavery in the early Cape Colony, the Afrikaners on the frontier developed attitudes toward Africans that persisted through the period of the Afrikaner republics of the nineteenth century to the current era of apartheid. Commandos (what Americans called posses) on the frontier retrieved stolen cattle and then began to procure juvenile agricultural labor. Later adult labor was also coerced, but, since the trekkers were not always more powerful than the Africans with whom they competed for land and labor, the frontiersmen largely turned to their government for help in procuring labor. This pattern has persisted. So has the notion that Africans existed in order to work for whites and that economic ascendancy in South Africa would result more from competitive strife than from cooperation between and joint exploitation of available resources by blacks and whites.

This is a successful book, with three informative chapters by the editors, the best of which systematically compares the American and South African experiences. The evaluations of the American experience by Robert F. Berkhofer and Ramsay Cook equal the insight of the essay on South Africa by Giliomee. Two chapters by James Axtell and Richard Elphick, on the Christianization of the respective frontiers, are also of lasting value.

Although South Africa was not North America, historians of one can profit from study of the other. In that sense, as well as others, this is an unusually rewarding book. It might have tried to be more methodologically innovative (even elementary approximations of significance remain unexplored), and it might have focused more thoroughly on the end of the frontier in terms of the etiology of contrasting systems of reserves, but no student of the American West or of early modern South Africa can afford to ignore this collection of well-crafted essays.

ROBERT I. ROTBERG
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ARNOLD SMITH. *Stitches in Time: The Commonwealth in World Politics*. Assisted by CLYDE SANGER. Don Mills, Canada: General. 1981. Pp. xix, 322. \$24.95.

The Commonwealth of Nations is an association whose definition and impact are elusive, even to the attentive publics of member countries. This book goes a considerable way to demystify the Commonwealth, and does so in chapter and verse rather than in stately concepts.

Arnold Smith, a Canadian, served ten years as the Commonwealth's first Secretary General. He therefore not only was witness and actor in Commonwealth affairs but also helped to shape the Commonwealth's stylistic and substantive modalities. He figures prominently in his own account of the Commonwealth, but on balance with justification. The Commonwealth Secretary General's role by now is that of a prominent international civil servant, often operating outside the glare of publicity but no less significantly, and at times with more effect.

Smith writes that he construed his job as "trying to shape events in sane directions." He often complains about the caution and negativism of traditional, national government bureaucrats. He is openly critical of and impatient with some leading politicians, whom he names. He writes of Edward Heath as an "extremely stubborn man"; of John Gorton as "insensitive" and as being "prepared to be irrational"; of Indira Gandhi as looking for a "distracting issue" on which to run for re-election.

But Smith surmises that in a hundred years the Commonwealth will be regarded as Britain's finest international legacy. He approvingly quotes Pierre Trudeau to the effect that Commonwealth summit meetings are "graduate seminars for heads of government," and adds that the many institutionalized as well as ad hoc problem-sharing and mediating encounters serve to enlarge the vision of the Commonwealth's members. Indeed, he finds that it is the larger and more powerful Commonwealth nations, Britain included, that are in greatest need of candid consultations within the Commonwealth framework, since it is they who tend to feel the least need for it.

Smith's account has wide thematic and regional range, and includes the Biafran secession, the protracted and painful Rhodesian negotiations, Papua New Guinea's relations with private investors, and the birth of Bangladesh. Efforts by the Commonwealth Secretariat, the Secretary General personally, or the good offices of some national members have not always averted crisis or solved disputes. The effects of unresolved disputes have, however, often been mitigated, and a number of internationally divisive disputes have been kept within bounds or composed. As example, Smith cites Commonwealth lobbying of Britain to rescind a proposed arms sale to South Africa. Had Britain not been persuaded to change its mind then, Smith argues, not only would the Commonwealth's foundations have been vio-

lently shaken, but also Britain's own international influence would have been severely impaired in and out of Commonwealth circles.

The labyrinth of "Commonwealth diplomacy," whether relating to the association's own members or toward outsiders, is fascinating and instructive. With a membership approaching fifty nations, drawn from all regions and of diverse political composition, and now far removed from being Anglocentric, the Commonwealth has proved, not only seemed, to be a constructive force. It has been constructive not only through its ongoing network of technical and economic cooperation or its ventures into crisis management, but also in the way that it has evolved a consultative idiom unparalleled among more mechanically contrived international organizations, including the United Nations. The Commonwealth experience is impressive, and Smith tells the story engagingly.

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JEAN-PIERRE BARDOU *et al.* *The Automobile Revolution: The Impact of an Industry*. Translated by JAMES M. LAUX. Chapel Hill: University of North Carolina Press. 1982. Pp. xvi, 335. \$20.00.

This book is quite a remarkable achievement. It is the first scholarly work I know that attempts to survey the history of the automobile industry on a worldwide scale. It was initially published in France and it has four authors, Jean-Pierre Bardou, Jean-Jacques Chanaron, Patrick Fridenson, and James M. Laux. Yet the text goes along coherently, with none of the troubles that frequently accompany multiple authorship.

To write the history of the world motor-vehicle industry this comprehensively in three hundred pages of text was an ambitious undertaking, and the authors are to be complimented on the success they have achieved. Under the circumstances a certain amount of superficiality was unavoidable, but there is far less than might have been expected. The treatment of the American automobile industry follows conventional patterns for the most part, except that it brings the record up to date, carrying it to 1981. The most useful part of the volume for American readers will be the wealth of information on the growth of the motor-vehicle industries of Europe, including the communist countries. There is also interesting material on the Japanese and Canadian automobile industries and on the growth of automobile manufacturing in recent years in other parts of the world.

There are some intriguing insights and suggestions. For example, the authors advance the thesis that because of negligible tariffs European manufac-

turers in the early days of the industry were not as handicapped by limited markets as has generally been supposed; the primary advantage that the United States had was higher per capita income (p. 47). They refer to the impact of the automobile on highway construction and make the point, as others have done, that the German autobahns were built for civil rather than military reasons—to stimulate the economy and encourage motor-vehicle production.

Although the book is subtitled "The Impact of an Industry" and wisely focuses on the economic consequences of motor-vehicle manufacturing, the authors give some consideration to the social and political problems that have grown up around the automobile. It is enough to say that they have done so with a balance that is unfortunately all too rare in discussions of these issues. For this subject the two final chapters, "The Automobile Under Fire" and "General Conclusions" are well worth reading. The authors weave their way skillfully and dispassionately through the issues of air pollution, traffic congestion, highway safety, and energy conservation. Some of their observations are worth quoting: "The automobile has attracted blame for some of the frustrations and dissatisfactions of consumer society, and for which it has become the concrete symbol" (p. 285); "consumers are far from prepared to give up the automobile or even to restrict their use of it" (p. 289); and, as a fitting conclusion, "the automobile today is the victim of its own success—actually, the automobile is not the true focus of the complaints that are directed toward it; they really aim at certain public policies" (p. 293).

JOHN B. RAE
Harvey Mudd College

ANCIENT

DONALD KAGAN. *The Peace of Nicias and the Sicilian Expedition*. Ithaca: Cornell University Press. 1981. Pp. 393. \$27.50.

Donald Kagan has now produced the third of his projected four volumes on the history of the Peloponnesian War, following *The Outbreak of the Peloponnesian War* (1969) and *The Archidamian War* (1974). As in the first two volumes, Kagan takes as his basic outline the account of Thucydides, adding to it or modifying it by citation of other ancient sources (the usefulness of Plutarch is ably defended [p. 284 n. 64]) and drawing on recent scholarship. Kagan is of the opinion that the speeches in Thucydides may properly be treated as "honest attempts to produce some semblance of the arguments made in speeches that were actually given" (pp. 7–8),

applying this interpretation even to the Melian Dialogue of *Book V* (pp. 149–52).

On the other hand, Kagan is definitely within the school that distinguishes between an “editor” Thucydides and a “reporter” Thucydides. This stance is taken with particular forcefulness in connection with the disastrous Athenian expedition to Sicily of 415–13 B.C., the subject of the second part of Kagan’s book. In his final chapter, Kagan underlines his point “by presenting a brief recapitulation of Thucydides’ interpretation of the Sicilian expedition, then setting forth the different view that seems to emerge from his narrative and attempting to account for the differences” (pp. 362–63). The argument in this particular case is convincing. Thucydides clearly wanted to present the Sicilian disaster as “one of the many errors resulting from the decline in leadership and the growth of democracy that followed Pericles’ death,” yet his narrative just as clearly “suggests that the mistake lay in the execution of the campaign, not in the concept itself” (p. 360).

Thucydides, says Kagan, attempted to guide his readers away from what appears to have been the consensus of his contemporaries, which blamed the disaster mostly on the Athenian general Nicias (pp. 369, 372). “Nicias, whose policy dominated [the period’s] first part, whose leadership dominated the second, and whose personality, talents, and flaws were so important for the shape and outcome of both” (p. 7), is seen by Kagan as the book’s unifying character. I feel, rather, that Alcibiades dominates the first part, which discusses developments from the Peace of Nicias in 421 through the battle of Mantinea of 418 and the Melian atrocity of 416. Kagan is certainly correct, however, in emphasizing that Alcibiades’ claims and pretenses greatly exceeded his accomplishments during all these years (pp. 254–59), and he is probably correct in seeing Thucydides himself as one of those taken in by his “unique personal charm and persuasiveness” (p. 256). The essentially diplomatic orientation and limited military commitments involved in Alcibiades’ policies, both as practiced on the Greek mainland (pp. 71–74, 83–84) and as envisioned in Sicily (pp. 213–14), are stressed by Kagan, and this viewpoint seems preferable to the common tendency (which Thucydides perhaps encourages) to view Alcibiades as rabidly and almost limitlessly imperialistic.

It is difficult to assess the extent of originality in a work such as this, which follows the organization of an unusually full and reliable ancient author and takes cognizance of a great amount of detailed scholarship. The readability and clarity of Kagan’s narrative are definite pluses. The book is accessible to any educated reader, with discussions of textual problems and untransliterated Greek passages rele-

gated to the ample and helpful footnotes. The many ironies of the career of Nicias, whose efforts to evade or lessen conflict repeatedly led instead to its escalation, are effectively conveyed (for example, pp. 189–191, 356, 372). Strategy and tactics associated with battles, notably Mantinea (chap. 5) and several engagements at and near Syracuse (chaps. 10–14), are particularly well explained, and well illustrated by excellent maps. Points of topography will always be disputable, of course. The omission in the maps of certain toponyms mentioned prominently in nearby text or notes is sometimes annoying; in map 11, two different Syracusan walls are labeled “first.”

The emphasis that Kagan places on the operation of factions within the Greek states is striking and unusual. He makes references to factions, parties, oligarchs versus democrats, and so forth in Athens, Sparta, Corinth, Argos, Melos, Messina, Thurii, Leontini, Catania, and Syracuse at various points in the book (some repeatedly). Such observations at times contribute to the narrative’s plausibility, as when he proposes that certain dissident Spartan *ephors* were the Argives’ source of information on the destruction of Panactum by the Boeotians (p. 58 n. 84), or when he discusses possible collusion between the Spartan king Agis and oligarchic generals in Argos (pp. 98–102). Sometimes, however, other explanations might work as well as or better than the unattested factional struggles Kagan suggests at, for example, Corinth (pp. 36, 43), Athens (pp. 71, 103), Sparta (pp. 29, 54, 56, 92, 139, 289), and Syracuse (p. 267). This apparent tendency to do more than is genuinely possible for the ancient historian is also striking in comments such as: “The ancient writers give us no direct account of Corinthian thinking about these developments; Thucydides merely reports Corinth’s actions” (p. 53), and “As usual, Corinthian thinking is not disclosed by the ancient writers, so we must speculate” (p. 141). Would ancient speculation be necessarily more valid than our own?

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VIRGINIA HUNTER. *Past and Process in Herodotus and Thucydides*. Princeton: Princeton University Press. 1982. Pp. xviii, 371. \$27.50.

Virginia Hunter in her earlier book, *Thucydides: The Artful Reporter* (1973), argued that the narrative of Thucydides was contrived in such a way as to support a thesis, and now, fortified by a study of modern theories of historiography, she returns to the attack with a book that proposes to compare the critical methods or methodologies, as she calls them, of Thucydides and Herodotus.

In the first half of this book, "The Past: Enquiry and Interpretation," she attempts to compare the attitudes of the two historians toward very early times by noting first how Thucydides writes in the *Archaeologia* in Book 1, where he appeals to Homer as a witness, and then how Herodotus reacts when he hears, in Egypt, of an Egyptian version of the story of Heracles that differs from the standard Greek version. Comparison of these two passages leads her to believe that Thucydides has a different attitude toward mythology than Herodotus, that he is less critical in fact. She does not warn the reader that elsewhere Thucydides (for example, II 15) and more frequently Herodotus (for example, IX 73) refer to mythical episodes and characters without qualifying their remarks, just as we very often speak of Shakespearean characters as though they were real persons, without revealing any kind of critical attitude or methodology. Neither writer shows any serious or consistent interest in efforts to discover the "correct" versions of mythical events. Herodotus felt himself obliged to express an opinion only when he heard the unusual Egyptian version of a story and Thucydides only when he thought it worthwhile to notice that the *Iliad*, if read correctly, supported his own view. The more proper contrast is not between Thucydides and Herodotus, but between them and mythographers like Hellanicus, Pherecydes, and Acusilaus.

In the second part of the book, "The Process of History," the author seems to be developing and expanding the theme of her earlier book. She maintains that when Thucydides describes the successes of Brasidas in Thrace he is really trying to explain in general terms the process by which an empire is undermined by *stasis*, when factions within a city are prepared to welcome foreign intervention to help them seize power. This is not so much a reasoned argument as an assumption. Because the narrative leads the reader to a generalization, it is assumed that it is specially designed and the material specially selected to establish a general conclusion. This might be a fair criticism of Thucydides' *Archaeologia* or of the narrative in Diodorus when it is based on Ephorus or Timaeus, who had no personal knowledge of the events that they described and were not personally involved in them. But it is hardly a fair assumption to make of the tale told by Thucydides, who held a military command in Thrace at the time, and was as well qualified as anyone to know what actually happened and what Brasidas actually said in his overtures to the cities. The author goes on to compare the Thucydidean narrative with the description in Herodotus of Darius's Scythian campaign, which is also considered to be a presentation of a "process."

This book deserves attention for its attempt to apply modern theories of historiography to He-

rodotus and Thucydides, but the mode of argument that the author uses in comparing the two historians cannot be considered sound, and it is certainly likely to mislead readers who do not know the text of these historians as well as they should.

LIONEL PEARSON
Stanford University

W. KENDRICK PRITCHETT. *The Greek State at War*. Volume 3, *Religion*. Berkeley and Los Angeles: University of California Press. 1979. Pp. 353. \$27.50.

This, the third volume of W. K. Pritchett's series on warfare in ancient Greece, is devoted to sundry aspects of the role of religion. There is no introduction or general conclusion, so that one must search for the theme of the book. There is furthermore no particular order of arrangement of the chapters save for the first, "Religion and Greek Warfare"; this forms a rough introduction to the rest by marshaling evidence to argue that religion was of cardinal importance in Greek society and thought, despite the skepticism of certain modern scholars who have explained numerous events of Greek military history on political, economic, or virtually any other grounds in preference to the religious explanations given by the ancient sources. Pritchett's principal theme is that the Greeks believed in the gods, their signs and oracles, and acted on these beliefs, and that modern scholars must not be loath to accept this reality if they are to understand the Greeks on their own terms.

Pritchett buttresses this theme by examining several topics, for example military epiphanies, in which a divinity was supposed to have intervened to assist one side in a battle; the military *mantike*, where the crucial significance of the seer's role in military decisions is underlined; and military oracles. In discussing Herodotus' story that the exiled Alcmaeonids bribed the Delphic oracle to pressure the Spartans to overthrow the Peisistratid tyranny, Pritchett rejects the critical view of Parke-Wormell, who wrote: "It (Herodotus' version) is rather naive in that it supposes that the Spartans could be induced by persistent pressure from Delphi to adopt a course of action without other motive"; he asserts, "On the contrary, there was no more powerful pressure than that from the god" (p. 309), and he is likely to be right.

This volume is a rich and sometimes esoteric collection of material, which displays a singular command of both literary and epigraphic sources; the material, on the face of it, is a bit hard to organize and arrange thematically, and the lack of a subject index is regrettable for it detracts from the book's usefulness. There are indexes to the ancient

authors and to the inscriptions cited, but these are manifestly useful only to those who are already familiar with a particular passage and wish to learn whether it is discussed. The reader who is interested, for example, in earthquakes as divine portents and their effects on military activities must turn to the chapter on "Miscellaneous Portents" and review the cases listed there. There is no general bibliography either, although the various chapters usually begin with brief discussions of the relevant and most important modern literature on the subject; and here the author collects references to entries in the several learned encyclopedias, dissertations, monographs, and periodical literature.

After a discussion of ancient passages bearing on a topic (where these exist), there usually follows a catalogue of the known examples, arranged on chronological grounds, together with Pritchett's commentaries on this material. The examples begin with the archaic age, or even with the Homeric corpus, when evidence for a particular topic can be found there, and stretch through the classical age to the late Hellenistic and sometimes even to the early Roman period; distinction is made between Greek and Roman practices, however, and the latter topic is excluded from coverage. Thus, the book represents a comprehensive treatment of its subjects. It is equally broad-ranging in subject matter, as the titles of the following chapters, among the longest and most important, will suggest: "Military Epiphanies," "The Military Mantike," "Military Portents," "War Festivals and the Calendar," "Dedications of Armor," and "Military Oracles."

In short, this volume brings together a diverse and interesting group of studies that may be consulted with profit by many who wish to deepen or expand their understanding not just of Greek religion but of Greek life and thought more generally.

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GRAHAM WEBSTER. *Rome Against Caratacus: The Roman Campaigns in Britain, A.D. 48–58*. Totowa, N.J.: Barnes and Noble. 1981. Pp. 181. \$17.50.

This work completes Graham Webster's trilogy on the Claudian invasion of Britain and the early stages of the Roman conquest, the other two books being, *The Roman Invasion of Britain* (1980) and *Boudicca: The British Revolt against Rome, A.D. 60* (1978). The reader will find it very helpful to have these accessible when using the present volume.

Caratacus is a detailed examination of the archaeological and literary evidence for the Roman campaigns and military dispositions in Britain in this period. There is a brief historical introduction; a chapter on the battle in which Caratacus was finally

defeated and its surrounding circumstances; and investigations—governorship by governorship, site by site—of military developments under the second, third, and fourth governors (Ostorius Scapula, Didius Gallus, and Veranius). Appendixes provide translations (by Mary Beard and Chloe Chard) of Tacitus, *Annals*, 12, 31–40; *Agricola* 14; Suetonius, *Nero*, 18; and Dio, 62.2.1; an essay by Antony Barrett on Cogidubnus; notes on the army in the Claudian period, the "Seven Sisters" bronze hoard, military tombstones from Wroxeter, and Romano-British place-names; and two glossaries. In passing, it should be said that the piece on Cogidubnus by Barrett, while stimulating thought on this difficult subject, contains much that is by no means generally accepted.

Webster's book will be of great use to the specialist in this particular field but hard going for the general reader, whether historian or classicist, who lacks a close knowledge of the topography of southern Britain. All readers, if they are not to be misled, will have to appreciate that Webster is following the modern technique of building "models" from the fragmentary evidence available, to stand as hypotheses for testing against future discovery and research. Careless use of the maps constructed by the author or the descriptions of military routes deduced and numbered by him is likely to lead to the erection of unsound theory. Close attention to Webster's text, however, will reveal the exact degree of certainty in the evidence behind each suggestion. The orderly collection of material in this fashion is valuable, and the book is full of interesting ideas worth pursuing.

Webster is strongest in his grasp of military detail and in his knowledge of the archaeological evidence on the ground. His touch is less sure in political and social matters. One may, for example, point to his notion (pp. 24–25) that the creation by Claudius of two "client-kingdoms" at the time of the invasion would have been "quirky." This ignores Julio-Claudian imperial flexibility, and the convenience where native aristocracies were too unreliable or insufficiently advanced for the early formation of local councils on which to off-load as much as possible of the burden of administration. Nor does the present reviewer share Webster's conviction that in Britain the Druids played a major role in resistance to Rome, a notion commonly based on dubious use of literary evidence relating to Gaul.

In sum, this is a book for the shelf of any Romano-British specialist.

PETER SALWAY
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SHAYE J. D. COHEN. *Josephus in Galilee and Rome: His Vita and Development as a Historian*. (Columbia Stud-

ies in the Classical Tradition, number 8.) Leiden: E. J. Brill. 1979. Pp. xvi, 277. f 96.

Shaye J. D. Cohen's dissertation is the single most important critical work on Josephus since that of Richard Laqueur, *Der jüdische Historiker Flavius Josephus* (1920). This splendid work signals a major step forward in the study of the principal problem presented by Josephus's *oeuvres*, the relationship between his *Jewish War*, written earlier in his career as a postwar memoir, and his *Jewish Antiquities*, coming somewhat later, along with his autobiography, the *Vita*, an appendix to the *Antiquities*. Cohen addresses head-on the disagreements between Josephus's *War* and *Antiquities*, providing an account of the relationships of the two. He shows that the *Vita* and the Galilean narrative of the *War* "can be understood only after we have investigated the motives and techniques of all of AJ [*Antiquities*] and BJ[*War*]." Cohen thus presents a study of Josephus's "development as historian, apologist, and Jew."

The work unfolds in a systematic and logical way. Cohen first presents a complete account of the contradictions between the *Life* and *War*. Second, he takes up the issue of "how Josephus treated his sources and examines the relationship of the first book and a half of BJ [*War*] to books 13–20 of AJ [*Antiquities*]." He next applies the results to the relationship of the *Life* and the *War*. He turns to a very detailed and careful analysis of the content of, first, the *War* (chap. 4) and then the *Life* (chap. 5). All of this is done in exquisite detail, with imagination and discipline, stunning erudition, and thoroughly persuasive results.

The final chapter shifts in tone and level of discourse. It is no longer a closely reasoned, detailed account of specific passages, but "a historical reconstruction." Cohen states at the outset that we cannot construct an account either of the Jews' war against Rome or of Josephus's place in it: "Because Josephus is our only extensive source and because he is so unreliable, our knowledge is very defective. That Josephus provides enough data to refute his own account is a sign of sloppiness. . . . The narrative is always tendentious and, because we have no external control, we can never be sure of the underlying events. . . . Thus it is easy to destroy Josephus's account, but nearly impossible to construct a more truthful one." After making that statement, Cohen proceeds to a rather elaborate fifty-page account of exactly the subject he claims he cannot take up. The general tone is captured in the following (p. 221): "In sum, although we do not have enough information to understand the details, the outline is clear." Over and over again, we move from what Josephus says happened to what was really going on. The result is that we really have two books before us, one a work of massive erudition and a pure, critical

spirit, the other a somewhat subjective set of judgments about what, in Josephus's stories, contains a kernel of truth or permits us to approach narrative history.

In the balance, Cohen gives us a superb piece of work, to be sure bearing a massive flaw. But the book's detailed work of close analysis of sources is spoiled in no way by the use of the results of the analysis in what I think is an inappropriate way. And the account of Josephus in Galilee, on its own, is by no means gullible or uncritical; it is as credible an account as anyone is apt to create. Still, Cohen's judgment at the outset, cited above, is correct, even for his own reconstruction of events. Let the last word be one-sided: a brilliant, imaginative, learned book, of the first rank, the single most important contribution to its subject in over half a century.

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POLYMNIA ATHANASSIADI-FOWDEN. *Julian and Hellenism: An Intellectual Biography*. New York: Clarendon Press of Oxford University Press. 1981. Pp. vii, 245. \$49.50.

Others will have to explain why, in the space of the six years from 1975 to 1981, no less than four biographies of the emperor Julian were published in the English-speaking world. The volume under review is the latest of these and by no means the worst. It is the outcome of doctoral research at Oxford University by a native Greek working under the supervision of a British scholar who is justly respected for his studies of late antiquity. Polymnia Athanassiadi-Fowden graciously acknowledges her debt to John Matthews in her preface and in so doing calls the reader's attention to what is the single most distinctive feature of her work: "I could never thank adequately Dr. J. F. Matthews of Queen's College, Oxford, for his iron determination to make me capable of putting my ideas across to an English public." Fowden's warm and occasionally passionate support of Julian's cause, together with a dithyrambic style that is often ill suited to the constraints of English prose, gives to this book an air of hagiography. Greek is calling unto Greek and regretting what might have been. And yet, at the same time, there is sound documentation in the footnotes and a kind of sad recognition of Julian's weaknesses that one finds in Ammianus Marcellinus. If a contemporary admirer of the emperor, like Ammianus, had to admit flaws, a modern admirer could do no less. It must have required iron determination to elicit these admissions. The tension between Greek enthusiasm and British scholarship permeates Fowden's book from beginning to end.

Fowden's study bears the subtitle, "An Intellectual

Biography," and this is presumably meant to indicate that she did not intend a conventional political biography. Certainly her treatment lays principal emphasis on Julian's inner life. We are told so often that Julian was loving and sociable that it becomes easy to forget all the evidence to the contrary (not only the partisan testimony of Gregory of Nazianzus, which Fowden invokes very sparingly, but the writings of Julian himself). If Julian "was endowed with a capacity for love and devotion which he never missed an opportunity to express" (p. 40), why did Ammianus have to fault him for *levitas* (fickleness), as Fowden herself observes, on page 83, without translating the word? Nor does Fowden face up to the hypocrisy of Julian's public profession of Christianity for ten years after his conversion to paganism. She has particular trouble in explaining away the blatant sycophancy of Julian's first panegyric to Constantius. No one is likely to credit her opinion that by following the established rhetorical precepts for composing panegyrics of rulers, Julian saw "the only way of absolving himself from the charge of lying" (p. 61). Fowden is similarly innocent in her handling of the soldiers' acclamation at Paris whereby Julian stood revealed as emperor in direct contravention of Constantius' wishes. Despite the research of scholars on both sides of the Atlantic, she continues to accept Julian's tendentious account of how greatness was thrust upon him.

It is perhaps in her repeated justification of the educational policy of Julian that Fowden's work is most disturbing. At the beginning of her book and then later in its proper place she cites with approval Julian's view that teachers should be allowed to teach only what they actually believed. This was, of course, the basis of his ban on Christian teaching of the pagan classics: "I give them (the Christians) this choice," wrote Julian: "either not to teach what they do not take seriously or, if they wish to teach, to practise what they preach." Fowden finds Julian's law on education "rich in feeling and crystalline in its logic" (p. 1). A state controlled by such logic is terrifying to contemplate, and Gregory of Nazianzus and other Christian masters of pagan learning had every reason to complain of it. Although Fowden sees great wisdom in Julian's imperial policies overall, she is compelled to grant some deterioration in his judgment near the end, and in this she draws close to many other biographers. There is really no other way to explain the disastrous Persian expedition. After the failure of his operations in Antioch, he seems to have undertaken his eastern war in a spirit of desperation and bravado. He courted death east of the Tigris, perhaps even sought it. At any rate that is where he found it.

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MEDIEVAL

HENRY A. MYERS. *Medieval Kingship*. Assisted by HERWIG WOLFRAM. Chicago: Nelson-Hall. 1982. Pp. ix, 467. Cloth \$25.95, paper \$13.95.

Medieval Kingship is a study whose content is more accurately delimited in the subtitle that appears on the dust jacket but not in the book itself: "the origins and development of Western monarchy in all stages from the fall of Rome to the fifteenth century." In the preface, Henry A. Myers and Herwig Wolfram cite their purpose as "to show how and why kingship came upon the scene of Western civilization to dominate political reality and popular imagination for well over a thousand years" (p. vii). Later they say "Our aim . . . has been to illustrate the development of kingship in the West in both fact and theory" (p. vii). They undertake a formidable if not impossible task, and yet, although in many ways the result is a conventional historical survey of the surviving written sources for the evolution of medieval kingship and royal government, the extensive coverage of this material and the resulting comparisons produce a picture not readily obtainable in other works on the evolution of royal power and its relationship to the states over which it was exercised.

The second half of this work follows the usual interpretation of Western monarchy: the role of the feudal king vis-à-vis his vassals and the church; the extension of the concept of the king's peace and development of a royal justice superior to seigneurial justice; alliance with the rising urban classes against the feudal baronage; decline of baronial financial, military, and administrative influence; the influence of Roman law; and early evidence of the growth of a true national feeling. To this is added a chapter on contemporary theory as reflected in the *Book of Emperors* and the works of John of Salisbury, Thomas Aquinas, Dante, Marsiglio of Padua, and Sir John Fortescue. All this is very well done, although it adds nothing new except insofar as Scandinavian material is added to the more usual French, English, German, Aragonese, and Castilian. (By sticking strictly to the title "kingship," most of medieval Italy can be ignored!)

The authors' more original contributions come in the first half of the book where they try to untangle the Germanic, the Roman, and the ecclesiastical contributions to early medieval kingship. There is a considerable surviving literature from the late Roman or early Christian point of view; using this material, our authors provide a very convincing treatment of the Roman and early Christian role in forming the new political organisms that replaced the Roman empire in the West. They make a valiant attempt to evaluate the Germanic contribution also, but here they fail, for almost no contemporary

Germanic written evidence survives from the period before the migrations and the evidence is not extensive for the period of the early Germanic kingdoms. They accept an early type of Germanic tribal king (arguing on the basis of Tacitus, Jordanes, and later Scandinavian sagas) and then are faced with the problem of trying to explain why these essentially sacred kings gave way to warrior kings at the time of the migrations. But their sources are much too unreliable to support the firm conclusions drawn, and as a result early medieval kingship appears in a more certain light than is justified. Once the Germanic kingdoms were established, the authors proceed to work with better material—the Germanic law codes and the national histories. They do a better job with the literary material than with the legal. Only the Visigothic laws are treated in any detail and even here they mistakenly attribute the *Edictum Theodorici* to Theodoric the Ostrogoth. Even had they investigated the so-called barbarian codes further (and the Lombard and Frankish laws contain considerable material on the working of their respective judicial systems), this part of the Germanic codes is not reliable for *original* Germanic ideas about the role of the state in matters of justice since the codifications were accomplished by Roman legal-trained personnel.

The resulting picture of the early Germanic kingship is thus not very convincing, but, aside from this, Myers and his collaborator have produced a useful survey of medieval kingship and its relationship to the development of the nation-states of early modern Western Europe.

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JEFFREY RICHARDS. *Consul of God: The Life and Times of Gregory the Great*. Boston: Routledge and Kegan Paul. 1980. Pp. ix, 309. \$25.00.

Jeffrey Richards's biography of Pope Gregory I (590–604) is complementary to his well-received *The Popes and the Papacy in the Early Middle Ages, 476–752* (1979). In sixteen short chapters he introduces the reader to the times of Gregory (pp. 4–24) and the pontiff's life. He also gives a brief description of the pope's attitude to the world and then turns specifically to a discussion of Gregory's pontificate, describing the pope's circle of trusted advisors, the administration, relationships with the episcopate, the Lombards, the West, the East, his missionary activities, and his importance for monasticism. He concludes with all too brief a reference to the legacy of the great pope (pp. 259–66).

The volume is intended as a fresh appraisal of Gregory's reign in areas where F. H. Dudden's biography (1905) has been superseded by more

recent scholarship, but the author disclaims the intention of replacing Dudden's volumes. Richards's work is distinguished by clarity and written in a refreshingly simple and direct style that will certainly appeal to the general educated public. The black-and-white illustrations (twelve pages inserted between pages 150 and 151) are well chosen to introduce the reader to the time of Gregory the Great. Annotations have been limited to a bare minimum and are relegated to the back of the book (pp. 269–91). It is in keeping with the tenor of the book that the author does not spend much time on scholarly controversies. He either dismisses them as insignificant or confidently takes his stand on either one side or the other without encumbering the text with intricate reasoning. For a more penetrating discussion of Gregory the Great, especially his thought and theology, scholars will wish to turn to Claude Dagens's study (1977) or to the volumes by Dudden. The single most regrettable lack is a map that shows both the Latin place names, which the author uses to "convey something of the feel of a world still rooted in its Roman past" (p. 3), and their modern equivalents—if any. A nonspecialist reader is thus lost without the use of dictionaries.

The most notable scholarly contribution of the book is the detailed and relatively technical discussion of Gregory's relationship with the episcopate of the ecclesiastical provinces under Roman jurisdiction (pp. 140–80). Using Gregory's letters, which have been preserved in his registers, Richards analyzes very skillfully how and why Gregory came to interfere with the canonical right of the clergy and people of a diocese to the election of their bishop. He shows both the pontiff's political and diplomatic skills as well as his determination to assure the influence of Rome in the various sees. At the same time Richards demonstrates that Gregory as pope had little choice but to act as he did. The book also succeeds very well in the discussion of Gregory's position both as pope and Roman citizen between the aggressive Lombards on the one side and the exarch of Ravenna with his mutinous armies on the other. As Richards points out, much of the temporal power of the papacy accrued to it at the time of the Lombard invasions, because the papacy had to step in to make up for Constantinople's inability to protect and provide for the population of Italy. This fact is well known, of course, but it is nevertheless notable with how much subtlety and insight Gregory's negotiations and interventions are described in the book. No reader will be left with the misconception that Gregory followed a preconceived blueprint for papal monarchy.

There is a great deal, therefore, to recommend the volume, but it must be said nevertheless that the almost complete neglect of the intellectual achievements of Gregory the Great, who is justly consid-

ered a pivotal figure between ancient and medieval Christianity, is a definite drawback. It is puzzling how the author can claim in a brief sentence that "the medieval dominance of Augustine's ideas derived from their Gregorian manifestation and prevailed through the Gregorian transmission" (p. 264) without explaining what he means. When the quoted statement is taken at its face value nobody who is at all familiar with medieval florilegia found in innumerable manuscripts will be able to agree with him. The error might seem small, but as a consequence it is—with the exception of England—never made clear what Gregory's legacy actually was and why he is such an important figure in medieval life and thought.

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JEREMY COHEN. *The Friars and the Jews: The Evolution of Medieval Anti-Judaism*. Ithaca: Cornell University Press. 1982. Pp. 301. \$22.50.

This is an important and stimulating book that offers a new perspective on the intensification of antisemitism in late medieval Europe. Jeremy Cohen's essential argument is that Dominican and Franciscan theologians rejected Augustine's position that the Jews must be tolerated as guardians of the Hebrew Scriptures who continue to observe the letter of the law. In reality, said the friars, medieval Jews have distorted the text of the Hebrew Bible and have in any event replaced it with a new law called the Talmud; to make matters worse, this Talmud is replete with perversities and blasphemies that make the retention of Jews in Christian society all the more dangerous. This elimination of the theoretical underpinning for toleration of Jews served as "a vital prerequisite" (p. 245) for the expulsions and persecutions of the late Middle Ages.

Cohen's demonstration of his thesis takes the form of a learned and vigorous analysis of the major attacks against Judaism and the Talmud from the investigations of the 1230s through the early fourteenth century. The disputations at Paris and Barcelona, Raymond Martini's *Pugio Fidei*, the inquisitorial activities of Bernard Gui, and the polemical works of Ramon Lull and Nicholas de Lyra are all subjected to fresh and often revisionist scrutiny. The results are always interesting and rewarding and sometimes particularly subtle and persuasive (as in the discussion of Lull's *Book of the Gentile and the Three Wise Men*).

The changes that Cohen brings to our attention are for the most part real and significant; it is not clear, however, that they add up to the fundamental reassessment of the Jews' status which is the burden

of the book's argument. At one point, Cohen concedes that the friars "may have paid lip service to the canons" tolerating Jews (p. 97); this concession, however, is obscured in the bulk of his discussion, which presumes a fully conscious and consistent formulation of a policy denying Jews a place in Christian Europe. The perception of the Talmud as a new law replacing the Bible gives way in most of the medieval formulations to an emphasis on its insults and blasphemies (pp. 92–95 on Gui; pp. 204 and 225 on Lull; p. 239 on Giordano da Rivolta). This is a serious matter indeed, but it does not strike at the very heart of the Augustinian theory of toleration. There is no real novelty in the argument that Jewish observance of the Old Testament is not "a legitimate *modus vivendi* before God" (p. 136); even Augustine did not claim that it was, and the use of this assertion as evidence of a radically new attitude significantly weakens the discussion of the Barcelona disputation (which is in any case excessively ingenious) and the analysis of the *Pugio*. In context, the accusations that Jews distorted the biblical text, referring as they often do to classical Christological verses, may not be as novel or significant as they seem at first glance. On occasion, Cohen asserts that Jews were denied a place in Christendom where this conclusion clearly goes beyond any evidence that he has adduced (pages 189 and 195 on Nicholas de Lyra). Finally, the friars' inconsistent struggles toward a denial of legitimacy to Jews may be as much a symptom of growing antisemitism as a prerequisite for increased persecution.

One last footnote: Alan of Lille's reference to a rabbinic work called *Sehale* is no longer "a mystery" (p. 31). M. H. Vicaire's study of Alan's polemic cites a manuscript reading of *Scola Helye*. Since the Talmud ascribes the passage in question to the "school of Elijah," this issue can now be safely laid to rest.

The central thesis of this book is somewhat overdrawn; nevertheless, Cohen has produced a work of scholarship, vision, and insight that will make all of us think again about one of the central issues in medieval Jewish history.

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GIUSEPPE ALBERIGO. *Chiesa conciliare: Identità e significato del conciliarismo*. (Testi e Ricerche di Scienze Religiose, number 19.) Brescia: Paideia, for the Istituto per le Scienze Religiose, Bologna. 1981. Pp. 368. L. 18,000.

In 1415, with the Latin church divided between three rival popes, the Council of Constance moved to end the schism by decreeing that the council

"holds power immediately from Christ and that anyone of whatsoever state or dignity, even if it be the papal, is bound to obey it in matters which pertain to the faith, the rooting out of the said schism and the general reform of the church in head and members." This decree, known as *Haec sancta*, did enable the council to end the schism and was generally recognized as valid for over twenty years, until the conciliarism that it made possible was pressed to extremes by the Council of Basel (1431–49) and consequently opposed by the papacy. Ever since, the status of *Haec sancta* has remained in doubt: long a favorite of Gallicans, it seemed obsolete after the doctrine of papal infallibility was promulgated in 1870 at Vatican I; but after Vatican II came collegiality and with it a new conciliarism that looked back to *Haec sancta* for justification. Consequently, for the last twenty years, Catholic theologians have been disputing the status of *Haec sancta* as never before; and, because the participants in this debate all appeal to history, the conciliar movement, so long neglected by Catholic historians, has become the object of their intensive study.

In the present book, Giuseppe Alberigo, a historian of the theology especially distinguished as an authority on conciliarism and collegiality in the Middle Ages, gives his synthesis of these disputes and studies. In effect, he is seeking to rehabilitate medieval conciliarism, and this he does by distinguishing two forms—moderate and radical. Moderate conciliarism, Alberigo argues, was a movement, not a doctrine, that developed slowly as other means of healing the Great Schism failed. Its antecedents lay, as Tierney discovered, in the canonists and not in the heretical works of Marsiglio and Ockham. This acceptable form of conciliarism found its doctrinal definition in *Haec sancta*, which Alberigo decides not only was validly enacted but also remains permanently in force. The implications of this position are softened, however, by the intentions of the fathers at Constance, who, unlike their successors at Basel, never claimed anything more than a legislative role for their council, and that only as an emergency measure. According to Alberigo, this moderate conciliarism gave way in the 1430s to the radical, heretical conciliarism of the reformers at Basel, which is none of his concern. Instead, he ends his study with the *Concordia catholica* of Nicholas of Cusa, whom he sees as the true heir and exponent of the moderate conciliar tradition.

This is a work of history in the service of theology, but one nonetheless valuable for historians, both for its subtle reconstruction of the complex forces that molded the decree *Haec sancta* and for its extensive, sensitive analysis of that text. As befits a work not intended for specialists, the documentation is light, with critical bibliographical footnotes that facilitate further study. As an introduction, English readers

will still want to begin with Brian Tierney's lucid account of the early stages of the controversy: "Hermeneutics and History: The Problem of *Haec Sancta*" in *Essays in Medieval History Presented to Bertie Wilkinson*, edited by T. A. Sandquist and M. R. Powicke (1969).

RICHARD KAY
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PAUL SPECK. *Artabasdos, der rechtgläubige Vorkämpfer der göttlichen Lehren: Untersuchungen zur Revolte des Artabasdos und ihrer Darstellung in der byzantinischen Historiographie*. (Poikila Byzantina, number 2.) Bonn: Rudolf Habelt, for the Byzantinisch-Neugriechisches Seminar, Freie Universität Berlin, Berlin. 1981. Pp. 423.

Most readers who are acquainted with the subject of this book will already have been surprised by reading the bibliographical heading of this review. They will recognize Artavasdos as a Byzantine general who raised a rebellion against Emperor Constantine V that, according to received scholarly opinion, lasted from June 742 to November 743 and led to Artavasdos's seizure of Constantinople and temporary restoration of the veneration of icons. The only narrative sources for Artavasdos's revolt are about seven pages of the chronicle of Theophanes and three pages of the chronicle of Nicephorus. One might therefore reasonably wonder what could be said about Artavasdos that could justify writing a book of 423 pages about him.

In fact, the length at which Paul Speck has treated Artavasdos's revolt is defensible. For one thing, minus the foreword, bibliography, notes, and six appendixes that mostly treat aspects of the period that are unrelated to Artavasdos, the main part of the book takes up only 133 pages, which would be under 100 if the book were set in type rather than reproduced from typewritten copy. Further, Speck makes two principal points that are well worth making if they are correct: he argues that Artavasdos's rebellion began not in June 742 but in June 741, immediately after the death of Constantine's father, Leo III, and that Artavasdos did not restore icons. The reference in the title to Artavasdos as "the orthodox champion of godly doctrines," quoted from Theophanes, is thus comparable to the title of Speck's book *Die kaiserliche Universität von Konstantinopel* (1974), in which he argued that no imperial university of Constantinople existed in the middle Byzantine period.

With only two narrative sources, composed more than fifty years after the event, Speck is not able to present totally conclusive arguments, though everything he suggests has at least some plausibility. In the opinion of this reviewer, his case that Artavas-

Artavasdus's revolt began in 741 is cogent, while his contention that Artavasdus was not an iconophile is possible but not likely. Theophanes puts the start of the revolt in 742, but Nicephorus and the dating formulas of contemporary papal documents put it in 741, and Speck argues convincingly that Theophanes' date resulted from misinterpreting a defective source and that Artavasdus claimed the throne as Leo III's son-in-law as soon as Leo died. Both Theophanes and Nicephorus say, however, that Artavasdus restored icons while he was in power at Constantinople, and I am skeptical about Speck's conjecture that their reports go back to a misinterpretation of a mutilated notice that Artavasdus repaired the ravages of an earthquake that occurred in 740.

Although those without a strong interest in the subject may wish that the book's style were more economical, historians of the period should be glad to have a full and careful treatment of this problematical rebellion.

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RALPH-JOHANNES LILIE. *Byzanz und die Kreuzfahrerstaaten: Studien zur Politik des Byzantinischen Reiches gegenüber den Staaten der Kreuzfahrer in Syrien und Palästina bis zum vierten Kreuzzug, 1096–1204*. (Freie Universität Berlin, Byzantinisch-Neugriechisches Seminar, Poikila Byzantina, number 1.) Munich: Wilhelm Fink Verlag. 1981. Pp. xiii, 549. DM 68.

This book, which examines relations between Byzantium and the Crusader states from the First to the Fourth Crusades, is most welcome. Apart from J. M. Hussey's survey article in the University of Pennsylvania Press *History of the Crusades* (1962), there is no other general treatment of this theme; nor is there any general work on the Comneni more recent than the two volumes written by Chalandon and published in 1900 and 1912. The chief merit of Ralph-Johannes Lilie's work is that he examines Byzantium's relations with the Crusader states in the context of its relations with the Christian West. This is a fruitful approach and is particularly enlightening in the case of Manuel I whose aims have frequently been misinterpreted.

Lilie's thesis is that Alexius I and John II, by using force to secure recognition of their overlordship in the Crusader states, met with only fitful and partial success there, while alienating much Western opinion. Manuel, who initially adopted a similar policy, broke with this tradition in 1158 and sought instead to cultivate the friendship of the Crusader states while continuing to claim suzerainty over them. His Western policies made this necessary: he required the support of the Holy See and the Italian mari-

time communes to achieve success in Italy, but those powers had strong vested interests in the Crusader states. A policy of confrontation with the Franks in Syria would therefore have jeopardized Manuel's Western alliances. Similarly, Manuel intervened in Egypt in 1169 because he hoped to gain partial control over the Egyptian ports, so vital to Italian commercial interests, and thereby to bind the maritime communes more firmly to his empire. His policy met with a large measure of success: he restored an Orthodox patriarch to Antioch in 1165 (without, it might be added, damaging his alliance with the pope); King Amalric of Jerusalem, once strongly anti-Byzantine, voluntarily recognized his overlordship in 1171; and in Western Europe Manuel's former enemy, Louis VII of France, became his ally. After Manuel's death Byzantium soon reverted to a policy of hostility toward the Franks of Syria but was too weak to intervene there effectively.

This book makes a valuable contribution to a general understanding of twelfth-century crusader history, but it also contains much that is of interest to the specialist reader. Lilie argues persuasively that various pieces of evidence, chiefly from Alexius I's reign, should be redated; he has meticulously re-examined the controversial sources relating to Latakia in the years 1098–1105; and, inevitably, he has questioned received opinion about particular issues, such as the extent of Byzantine claims to overlordship in the county of Tripoli. Not all his judgments are equally convincing: for example, his contention that Raymond III of Tripoli led the pro-Byzantine faction in Jerusalem in Baldwin IV's reign is questionable. Controversy is, however, a stimulus to research, and this book will undoubtedly provoke much fruitful scholarly debate. It is no criticism to add that this work is concerned chiefly with political and diplomatic history and that much still remains to be done in this field about religious and cultural relations between the Crusader states and the Byzantine empire.

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JOHN GODFREY. *1204: The Unholy Crusade*. New York: Oxford University Press. 1980. Pp. xi, 184. \$37.50.

John Godfrey has written a book "intended primarily for the general reader," and in that purpose he has succeeded. His study, a straightforward narrative account of the Fourth Crusade, reads well, is based on a wide variety of primary and secondary sources, and does not devote an extensive amount of space to the usual scholarly apparatus of bibliography and footnotes. It is attractively illustrated with plates and maps, and the appendixes contain chronological and genealogical information of value in

identifying some of the principal characters of the "Unholy Crusade."

There is certainly nothing wrong with bringing an important or unknown story from the remote past to the attention of the public. But does the present state of scholarship on the Fourth Crusade really warrant another general study? No, and in this age of high-cost printing and other difficulties for historical publications it is difficult to understand why history editors continue to commission books on topics that have been adequately covered. Inevitably such books are repetitious, add little new, and, in some instances, detract from earlier but more scholarly work. There is, unfortunately, a little Whig in every historian and a new publication is often assumed to be "better" than those that precede it simply because it is more recent.

In the present instance I would have to recommend that serious readers interested in the Fourth Crusade rely on Donald Queller's *The Fourth Crusade: The Conquest of Constantinople, 1201-1204* (1977). It is as readable as Godfrey's book, more precise in detail, and more thoroughly documented. It also has an excellent discussion of the sources and literature on the subject and a lengthy bibliography. In addition, of course, there are the specialized studies in the second volume of *The Crusades* under the general editorship of Kenneth Setton. None of these is in any way superseded by Godfrey's account.

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BERNARD HAMILTON. *The Latin Church in the Crusader States: The Secular Church*. London: Variorum Publications. 1980. Pp. x, 409.

This work's principal value derives from its comprehensive survey of the establishment and subsequent fortunes of the secular diocesan Latin church in the four territorial products of the First Crusade and of the early twelfth century: the county of Edessa, the principality of Antioch, the county of Tripoli, and the kingdom of Jerusalem. Bernard Hamilton's analysis includes the Byzantine-papal diplomatic activity that launched the crusading movement and led, albeit unintentionally, to the foundation of the new churches in Frankish Syria; to the churches' territorial, corporate, and jurisdictional structures and their sources of income; to the careers and policies pursued by their clerical leaders; and to the Latins' ecclesiastical relations with the local Christian churches—the Greek Orthodox, Armenians, Jacobites, Maronites, and Nestorians.

Much of the data that Hamilton presents to illustrate topical categories of his ecclesiastical study is illustrative of the Crusader states' colonial experi-

ence and consequently will be of great value to students of what is recognized as a major phase in the development of Western colonialism. Byzantine-papal diplomatic relations yield details of the far-reaching Western military intervention in this area, which led to the Latin conquest and settlement. The structural and fiscal qualities of the new Latin patriarchates, dioceses, and parishes show how the ecclesiastical colonial architects adapted to local circumstances. The foreign origin of all Latin patriarchs of Antioch and of Jerusalem before 1187 and of many of the higher secular clergy in the twelfth century emphasizes the alien character of the new church. This social phenomenon also shows how "colonial service" in the Crusader states offered opportunities for advancement to Western clerical careerists. The ecclesiastical policies adopted toward the native Christian clergy and communities were among the most significant of the new states' colonial administration; not a few of these measures had some bearing on the cherished papal goal of ecclesiastical union.

Hamilton's study would have been somewhat enhanced by his taking into account the Hills' provocative biography of the secular protagonist of Latin-Greek cooperation on the First Crusade, Count Raymond IV of Toulouse (1962), and by his addressing more fully the problematic status of Archbishop Daimbert of Pisa as a papal legate (see John Gordon Rowe's article in *Speculum*, 32 [July, 1957]: 476 and note). Even so, Hamilton has admirably fulfilled the ambitious task he set for himself, and in doing so has provided not only a fuller picture of life in Outre-Mer, but a better foundation for appreciating the intricate problem of Christian unity with which men of that era and moderns still struggle.

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WILLIAM URBAN. *The Livonian Crusade*. Washington: University Press of America. 1981. Pp. x, 562. Cloth \$28.50, paper \$18.75.

HARTMUT BOECKMANN. *Der Deutsche Orden: Zwölf Kapitel aus seiner Geschichte*. (Beck'sche Sonderausgaben.) Munich: C. H. Beck. 1981. Pp. 319. DM 38.

WILLIAM URBAN. *The Prussian Crusade*. Washington: University Press of America. 1980. Pp. ix, 459. Cloth \$25.00, paper \$16.00.

The Order of Teutonic Knights and its role in the German conquest and colonization of the eastern Baltic region constitute the common ground that links these three books. While Urban's two volumes furnish sweeping narratives of the conquest, both of

Prussia in the thirteenth century and of Livonia in the two following centuries, Boockmann's work is more episodic and selective. Boockmann indeed deals with an even longer span of time than Urban. While Urban limits himself in the main to the period between about 1200 and the Reformation, Boockmann follows the fortunes of the Order through the age of religious reform and into the twentieth century.

Both authors are well abreast of the standard literature on the history of the medieval eastern Baltic, but the scholarly apparatus of Urban's two volumes is fuller and more helpful than Boockmann's relatively scanty annotations. Both authors include bibliographical essays in their books, and these are also useful to the general reader, who might easily go adrift in the confusing currents of Baltic historiography. Urban is one of the very few Western medievalists whose work reflects the results of recent Polish and Lithuanian research in medieval Baltic history. He is considerably more at home with their literature than Boockmann is. Boockmann does, to be sure, cite a few post-World War II Polish historians, but he takes little account of the Russian secondary literature and none of the work in the Baltic languages. Where Boockmann does deal with Polish treatments of the Teutonic Order, in the final chapter of his book, his discussion centers mainly on the nineteenth-century imaginative literature—Sienkiewicz and Mickiewicz especially—rather than on the twentieth-century historical scholarship that Urban draws upon.

Boockmann's overview of the history of the Teutonic Knights is cast in more or less conventional terms. After a brief introduction, he sketches the early history of the knights in the Holy Land and then moves quickly on to the beginning of the Order's involvement in Europe. Boockmann's special interest is clearly in the Prussian phase of the Order's history, and the greater part of his book deals with the Order's experiences in Prussia. Three fairly brief chapters outline the Teutonic Knights' conflicts with the Poles and the Lithuanians during the fourteenth century. One of Boockmann's most useful contributions in this book is his analysis of the internal structure of the knights' government of late medieval Prussia. His book concludes with discussions of the Order's fate in the post-Reformation period and of the modern historiography dealing with the knights. All told, this is a worthwhile piece of work, readable, and craftsmanlike, but not a major addition to the existing German secondary literature on the medieval Teutonic Order. It has the virtue of being serious, sober, and even-handed in its treatment of the knights' activities in the Baltic; but it does not make major original contributions to the history of the Order or the region.

The objective of Urban's two volumes is to pre-

sent a detailed narrative history of the thirteenth-century conquest of Prussia and of the completion during the fourteenth and fifteenth centuries of the German incursions into the eastern Baltic that began at the end of the twelfth century. There have been very few previous treatments of these themes in English, and none of the earlier ones attempts the kind of closely focused detail that Urban favors. Hence Urban's volumes make a substantial contribution to the English historiography of the late medieval Baltic—nothing else in the language even comes close to the kind of treatment that we have here. Beyond that, Urban's volumes are works of synthesis, for they bring together a body of information and ideas that have not been assembled in this way in any language, so far as I am aware. Urban draws upon a rich body of secondary work both in German and in the East European languages and manages to make remarkably good sense out of a very unwieldy mass of material. Add to this the author's demonstrated familiarity with a bewildering variety of printed primary sources, mainly in Latin, Middle High German, and Low German, and these two volumes constitute an impressive tour de force. Moreover, Urban is even more conscientious than Boockmann in avoiding nationalistic stereotyping. Indeed, Urban repeatedly warns the reader—and, I suspect, himself—against the invidious nationalistic biases of earlier generations of historians who have dealt with Baltic problems.

Having said all of this, let me note some reservations as well. First, Urban's narrative is as dense as it is detailed, with the result that it is often difficult to follow and occasionally confusing. Granted that the events that Urban deals with are often complex and that the primary sources are themselves not lacking in confusion and contradictions, nonetheless Urban's account sometimes does too little to clarify the issues in the episodes that he narrates.

Second, most English-speaking readers will require, I think, better guidance than Urban gives them to the geography and topography of the eastern Baltic. Both volumes include a few sketch maps, to be sure; but the cartography is crude, amateurish, and imprecise. Moreover, there is not even a list of maps to enable the reader to find help quickly when the underbrush gets too thick. This is more than just a nuisance: it is a considerable hindrance to the understanding.

Finally, a minor, but not insignificant, complaint about the indexes. They do indeed exist and one should be grateful for that; but they are largely limited to entries of proper names of persons and places and are nearly useless for referring back to topical and thematic items in the text. Making such an index is a dull and frustrating job; but the lack of one makes these two books considerably more difficult to use for reference purposes, which one can

reasonably expect will be precisely the reason why most readers will consult them.

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P. H. W. BOOTH. *The Financial Administration of the Lordship and County of Chester, 1272–1377*. (Remains Historical and Literary Connected with the Palatine Counties of Lancaster and Chester, third series, number 28.) Manchester: Manchester University Press, for the Chetham Society. 1981. Pp. xii, 207. £16.50.

The earldom of Chester was acquired by the crown in 1237; in 1254 Henry III gave the county to his son and from that time the county remained either in the king's hands or those of the heir apparent. By the early fourteenth century the earl's household and governmental officials were organized like those of any other magnate in England except for the use of clerks detached from the royal government for this purpose. These officials later became the government of England when the heir became king. For example, Walter Reynolds progressed from the service of the future Edward II in Chester to the posts of treasurer of England, royal chancellor, and archbishop of Canterbury. Probably the caliber of officials following this career pattern explains why the Cheshire accounts and associated records show a high standard for financial administration in comparison with the estates of other laymen. One of the most important documents on which this study is based is the *Black Prince's Register*, which contains copies of the letters issued under his privy seal. Because the local records are best preserved from approximately the same period covered in the register, P. H. W. Booth necessarily emphasizes the period 1346 to 1377, when the Black Prince and his officials ran the county.

In spite of the special connection with the monarchy, Booth concludes that the social and administrative history of Cheshire was much like that of counties in the North Midlands and can best be understood in conjunction with the larger area. Nor was Chester isolated from the influences that affected England as a whole. In fact, the financial administration was especially affected by the Welsh wars and, after 1346, by the Black Prince's obsession with the war in France and his particular interests in Gascony. Booth shows that the problems of financing the Black Prince's campaigns on the Continent distorted the financial administration of Chester and contributed to the separatism that became overt under his son, Richard II.

The author has chosen a carefully defined subject as described in the title of the book and has pro-

duced a thoroughly workmanlike piece of research valuable for the history of Chester. He has also read the historical literature on finance and estate management in medieval England and is able to add some minor contributions to it. In fact, his corrections to some generalizations made by other scholars, who have misinterpreted the highly technical format of some medieval accounts, illustrate his own careful approach and provide a salutary warning against the perils of hasty generalization. The section on minister's accounts and accounting procedures makes valuable reading for anyone attempting to use this type of document. Even though demographic figures for Chester are lacking for any far-reaching study, this book provides some additional information on the effects of the Black Death. The accounts of Macclesfield Manor provide a brief manorial case study. Although difficult to fit into the general pattern of English medieval history, this book will stand alongside others in modifying the usual interpretation of what the palatinate status of Chester meant in reality.

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A. C. REEVES. *Lancastrian Englishmen*. Washington: University Press of America. 1981. Pp. x, 419. Cloth \$24.50, paper \$14.75.

Lancastrian Englishmen is a collection of five biographies of men whose careers ran their courses during the first six decades of the fifteenth century. The biographies are of: Sir John Pelham (d. 1429); Henry, Lord Fitzhugh (d. 1425); Sir John Cornewaille, Lord Fanhope (d. 1443); Adam Moleyns, bishop of Chichester (d. 1450); and William Booth, bishop of Coventry and Lichfield and archbishop of York (d. 1464). All five rose to serve as royal councilors, and several of them held one of the great offices of state. Four of them came from county gentry families, and one, Lord Fitzhugh, was the scion of a baronial family ennobled in the fourteenth century. Cornewaille, a descendant of King John, was raised to the peerage; the reasons for this advancement, however, are obscure. Through the biographies of these men, A. C. Reeves attempts to show "successful career patterns" of the ruling landed class in fifteenth-century England.

Each biography is constructed from a vast number of facts that Reeves has mined from the contemporary sources. The published source materials have been ransacked with probably every nugget of useful information having been found. In addition, numerous references from manuscript materials, especially in the Public Record Office, London, to the five have been discovered and placed in the

biographies. Reeves's search has yielded such a great quantity of facts that little likelihood exists that subsequently discovered material will alter the picture he has drawn of each historical figure.

Nevertheless, these biographies are inadequate. Their basic fault lies in the materials from which they are compiled. The bulk of the information existing for each consists of grants made to or by each man. Even the register of Archbishop Booth is largely an impersonal record that tells us little about Booth himself. Repeatedly, Reeves admits that the existing records do not allow the historian to know what roles these royal councilors played in the creation of policy at the national level. Nor is the situation much better at the county level, for although each is appointed to various local offices or commissions almost nothing is said regarding the carrying out of these responsibilities. As a result these biographies read as hardly much more than a series of undigested note cards and remind one of the literary quality of the entries in G. E. Cokayne's *Complete Peerage*.

Having ferreted out and presented a vast array of facts concerning his five subjects, Reeves simply stops. No effort is made to explain what all of this information means for the understanding of the "successful career patterns" of the Lancastrian period. This lack of analysis is most unfortunate given the immense effort made to collect the facts that make up the remains of the lives of these five men.

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BARBARA H. ROSENWEIN. *Rhinoceros Bound: Cluny in the Tenth Century*. (Middle Ages.) Philadelphia: University of Pennsylvania Press. 1982. Pp. xxii, 173. \$18.00.

Scholars have studied Cluny's history during the tenth century less intensively than the ages of Abbots Hugh of Semur and Peter the Venerable; Barbara H. Rosenwein's essay therefore fills an important gap. Her metaphor of a "rhinoceros bound" comes ultimately from the Book of Job but immediately from the writings of Cluny's second abbot, Odo, who adapted it to portray the powerful man set under discipline in order that he might crush the oppressors of the humble. She regards the metaphor as applicable both inside and outside the cloister. Inside, the Cluniacs disciplined individual idiosyncrasy by incorporating it in a daily round based on law, and coercive power could be used to compel the recalcitrant to follow the Cluniac way of life. Outside, Cluny's tenth-century benefactors shared an experience of social dislocation that caused many people to drift both upward and downward in society and to look for discipline.

Cluny stood for stability. Within the monastic order, the emphasis on restraint, lawfulness, and prescribed and ritual behavior provided an antidote to the prevailing dislocation; in society generally it set a limit to violence and injustice as well as fostering virtue and patience.

Rosenwein's study has solid merits. She opens with a concise and critical account of recent Cluniac studies. She then considers historians' views of Cluny's tenth-century benefactors, rightly noting how it was under Count Otto-William of Mâcon (982–1026) that social disintegration in its locality became accelerated and that Cluny received a dramatic increase in endowments. She makes good use of Abbot Odo's writings to explain Cluny's understanding of the world, drawing on his *Life of Gerald of Aurillac*, the exemplary layman. From John of Salerno's *Life of Odo* himself, she shows how Cluny developed in the cloister a ritualized order that found written expression in the eleventh-century customs. She thus displays Cluny in the round, keeping both monastic and social developments in view.

The main reservation that must be made is that Rosenwein somewhat overdoes the applicability to the tenth century of such sociological concepts as anomie, which Durkheim proposed as a means of explaining modern industrial societies: support of Cluny was, in her view, a socially constructive response to anomie. But her point would be established only if it were shown by a thorough analysis of donations to Cluny that, under Count Otto-William in particular, most of them were prompted by disorientation arising from rapid social rise or fall. It is by no means clear that Cluny's charters warrant such a conclusion. One is left with doubts whether categories devised to explain one age can usefully be transferred to a quite different one. Historical explanation is perhaps best attempted in language that arises directly from the evidence for the period under discussion.

Such reservations in no way affect the value of most of Rosenwein's study, in view of her normally careful and illuminating use of the sources. It is to be hoped that she will go on to write a full-length work on Cluny under its earliest abbots.

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ANDREW W. LEWIS. *Royal Succession in Capetian France: Studies on Familial Order and the State*. (Harvard Historical Studies, number 100.) Cambridge: Harvard University Press. 1981. Pp. x, 356. \$37.00.

The Capetian royal family, distinct from the institution of monarchy, has received scant attention from

historians of medieval France. Andrew W. Lewis demonstrates that an examination of the royal family as a family provides an important new perspective on the nature of the Capetian monarchy. By placing the kings within the context of family and lineage, Lewis reinterprets a number of royal policies as deriving from the king's position as head of family rather than from his office as king. Moreover, the royal family shared with other noble families common values regarding marriage, succession, and inheritance, as well as similar attitudes with respect to the free disposition of their lands, both inherited and acquired. In brief, the royal family behaved very much like a powerful noble family and not according to any model of royalty with national responsibility.

Applying the most recent findings on the evolution of noble families, Lewis analyzes the language and meaning of charters and chronicles in order to understand royal naming practices, principles of succession, and the significance of anticipatory association of the eldest son with the king. From the tenth century, when noble families began to think of themselves primarily in terms of lineage instead of extended clan, the eldest son received a patronymic and was assigned the entire patrimony, in contrast with Carolingian precedents. The Capetian dynasty shared those practices even before it acquired the throne in 987. Furthermore, anticipatory association of the eldest son with the father was neither unique to the royal family nor intended to guarantee Capetian claims to the throne, which were never seriously questioned after the tenth century; rather it was to assure succession to the patrimony and office by the eldest surviving son and to provide an orderly settlement of the family estate. The creation of appanages by written testament in the thirteenth century simply continued that practice of assigning acquired land to younger sons and binding them to the future king. Royal marriages were made for immediate needs, not with a view toward the long-term growth of the monarchy. According to Lewis the Capetian kings acted with familial rather than national interests in mind and the appanages failed to destabilize the kingdom only because the royal cadets, unlike most noble cadets, suffered an exceptionally high rate of mortality in the thirteenth century. Lewis concludes that the Capetian kings can be best understood as heads of family rather than heads of state and that the creation of a cohesive kingdom was due more to chance than to the vision or policies of the kings.

This study breaks ground for a major reassessment of the Capetian monarchy and its role in the formation of French national institutions. It will generate debate on a number of issues, of which two in particular will certainly require further consideration. First, the geography and chronology of inher-

itance practices must be determined more precisely, for they were not necessarily uniform nor based on primogeniture at an early date in all regions. Perhaps the Capetians were more regional in family practices and outlook than we have realized. Second, much more will have to be said about the development of the royal bureaucracy in the thirteenth century. The proliferation of officials, procedural requirements, written records, and internal administrative interests must have produced, if they did not result from, quite different perceptions of monarchy and monarch than prevailed earlier. Nevertheless, this book will remain one of the most original contributions in the historiography of Capetian France.

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S. H. CUTTLER. *The Law of Treason and Treason Trials in Later Medieval France*. (Cambridge Studies in Medieval Life and Thought, third series, number 16.) New York: Cambridge University Press. 1982. Pp. x, 272. \$47.50.

It is remarkable that, for all the work of medieval legal historians during the past century, no one has produced a general study of treason for any area until scarcely a decade ago when J. G. Bellamy's volume on late medieval England appeared. This is remarkable because treason concerns one of the two or three fundamental conditions involved in the relationships between king and subjects and because England and France are probably the only two states whose medieval records are adequate for such a study. S. H. Cuttler has now filled this important gap with his study of treason under the Valois kings from 1328 to 1494.

Because England and France are most susceptible to such study, one cannot avoid comparison. Cuttler shows at the outset that, although the feudal concern of infidelity sometimes affected a particular case of treason, the French kings and lawyers from the middle of the thirteenth century relied exclusively for their law of treason on the definition provided by the two Roman laws known as the *lex Julia maiestatis* and the *lex Quisquis* as interpreted by certain thirteenth-century civilians.

This law defined treason as lese majesty, an injury against public authority; the monarch represented this authority and the crown symbolized it. Lese majesty could be committed only against the king, the crown, and the realm. Ultimately, Louis XI felt that injured majesty was the central aspect of treason, and this view strengthened the law's potential for encouraging absolutism. Unlike England, therefore, France was never driven to develop an indigenous statute of treasons. In England the idea of

resistance to the crown, bound up as it was in feudal premises, flourished throughout the medieval period, but in France the concept of lese majesty presented an effective antithesis to the idea of resistance.

Cuttler's book is well balanced in organization and treatment. Roughly half is devoted to general questions, such as the concept and categories of treason and the jurisdiction, procedure, and punishment that were used. In the second half he presents an empirical analysis of the incidents of treason between 1328 and 1494. He has produced a concise and valuable treatment of the major questions even though he did not seek to exhaust the subordinate aspects of his subject.

There is no mistaking the historical event that determined the incidence of treason in late medieval France. From the beginning of the Hundred Years War the number of treason cases grew at a precipitous rate. Given the particular nature of the war, treason was its natural concomitant as nobles and towns in the disputed areas from Normandy and Brittany to the Mediterranean guessed repeatedly which of the two monarchs would be their future lord. The indirect effects of the war on treason can be seen in the tax revolts of towns and the conspiracies of the princes and great nobles. Severity of punishment was not often feasible, and royal leniency resulted in an impressive rate of recidivism. The continued high incidence of treason under Louis XI must be considered as largely the legacy of the prolonged conflict. Without the Hundred Years War, the study of treason in late medieval France would be inestimably more difficult.

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MARIE-THÉRÈSE LORCIN. *Vivre et mourir en Lyonnais: À la fin du Moyen Âge*. Paris: Éditions du CNRS. 1981. 85 fr.

During recent decades, numerous scholars who have wished to use advanced methodologies have studied the social history of Western Europe during the later Middle Ages, because from that time there exist for some regions sources suitable for quantification by computer techniques. In the volume under review, Marie-Thérèse Lorcin presents the results of such research on testaments from the Lyonnais from 1300 to 1510.

The book consists of four detailed chapters on demography, successional practices, family structures, and summary probings into religious, cultural, and "material" life. Numerous tables complement the text. The earlier sections are the most useful. In them Lorcin traces and contrasts the

patterns of city and countryside, of nobles and commoners, and of the different periods within a sequence of overpopulation, demographic plunge, renewal, and "baby boom." In general, her findings accord with what is already known from studies of other regions during those centuries. Her discussion of widowhood is original and valuable.

In other regards, the work is unsatisfying. Lorcin does not define the region that she treats. Instead, she programs for the computer a mass of testaments—approximately half of the 4,000 recorded at Lyons and perhaps 1,500 of the 11,000 recorded in Forez (pp. 5, 19, 36, 194). Her treatment of the computer tabulations is uneven. Some of her arguments are fully plausible; others are dubious, either because one does not know on how many documents they are based (the samplings vary) or because the tables lack necessary chronological or other differentiations. The bases for many of her figures are unclear, because the tables often list percentages without stating the numbers of testaments or persons involved, then give a mean based on one of these numbers, not the average of the percentages. Acceptance of such statistics rests on trust, but Lorcin's arithmetic does not always inspire confidence. In one passage she says that, before 1380, the four peasant testators at Saint-Symphorien-sur-Coise who had several sons all divided their estates, yet on a table she indicates that one-third of them named a single son as heir (pp. 48, 51), and elsewhere she states that eight is "a little less than one third" of twenty-one (p. 63).

A reader interested in her sources will be disappointed. Not only does the bulk of her material preclude itemized citation, but she gives no references for the documents from which she draws specific examples, and her endnotes cite fewer than two dozen works by other scholars. A broader criticism of method is that, although Lorcin records some variations in practice among selected towns or hamlets, she does not discuss, and places no stress on, the diversity thus documented. This is a questionable approach in a study of an essentially localized society.

In sum, the results are mixed. The author has assembled a vast amount of material, but this presentation of it must be used cautiously.

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KARL HEINEMEYER. *Das Erzbistum Mainz in römischer und fränkischer Zeit*. Volume 1, *Die Anfänge der Diözese Mainz*. (Veröffentlichungen der Historischen Kommission für Hessen, number 39.) Marburg: N. G. Elwert. 1979. Pp. xii, 237. Cloth DM 72, paper DM 62.

Charting the evolution and boundaries of dioceses in the early Middle Ages is notoriously frustrating work because the sources are few and niggardly. Such studies have already been done for the dioceses of Cologne and Trier, and this *Habilitationsschrift* was produced at Marburg, under the direction of Walter Schlesinger, to complement this earlier work by tracing the development of the see of Mainz up to the time of Boniface. (A second volume is planned to carry the study up to 919 and to include the missionary activity east of the Rhine.)

Like many other early medievalists nowadays, Karl Heinemeyer supplements the paucity of the literary evidence by using archaeology, place names, and patterns of settlement for his patient reconstruction. His findings may be briefly summarized as follows: The first reference to a bishop of Mainz occurred only in the 340s, and the late imperial bishops had jurisdiction over the three *castra* of Alzey, Kreuznach, and Bingen as well as the *civitas* of Mainz. This ecclesiastical district corresponded to no Roman administrative organization, urban or provincial, and with the collapse of the Western empire the bishopric evidently disappeared around A.D. 450 for about a century. The early medieval bishops resumed their Roman predecessors' authority and began to extend it. In this expansion they followed Roman roads and topography and were aided by the Frankish kings. This growth was blocked to the west in both the Roman and the medieval periods by the aggressive bishops of Trier, who by contrast enjoyed aristocratic rather than royal patronage. This detailed investigation of Mainz and comparison with Trier shows clearly that the development of early medieval dioceses was gradual, variegated, and influenced by a variety of factors, personal and impersonal.

These conclusions are important but hardly surprising. In fact, despite the high quality of the accompanying maps and plates, this is a disappointing *Habilitationsschrift*. It does not display the breadth, either in the topic chosen or in the learning marshaled, that one expects in the second German doctorate. It also suffers from some real flaws. It is anachronistic, for example, to search for a "diocese" of Mainz in late antiquity, when it is well known that this term begins to surface in the West only in the fifth century, and it is particularly off the mark to seek such an advanced level of organization in a Christian community that bloomed so late in an exposed and crumbling frontier province. At the same time, Heinemeyer, in his determination to find the diocese coterminous with either the *civitas* or the *provincia*, does not perhaps make enough of the fact, revealed by the *Notitia dignitatum* of ca. 400, that the areas subject to the bishop of Mainz were also subject to the *dux Mogontiacensis*.

As for the Frankish see, Heinemeyer is a trifle too

zealous in associating its refoundation and advancement with the crown rather than the nobility. It may well be that the Roman-Frankish aristocracy early gobbled up control of the more settled areas around Trier and thus forced the Merovingian kings to work harder to establish a base around Mainz, but Heinemeyer does not explore this possibility. Instead, to connect king and bishop he uses words like "offenbar," "vermutlich," and "Eindruck," particularly with regard to the revival of the bishopric. One suspects an excess of filial piety for "royal" Mainz as opposed to merely "aristocratic" (and bellicose) Trier. Finally, one suspects, too, that Heinemeyer's sources, however meager, would have yielded much more had he put to them the sorts of interesting questions that Friedrich Prinz and other German early medievalists have been doing for several decades now.

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HANS-EBERHARD HILPERT. *Kaiser- und Papstbriefe in den Chronica majora des Matthaeus Paris*. (Veröffentlichungen des Deutschen Historischen Instituts, London, number 9.) Stuttgart: Klett-Cotta. 1981. Pp. 241. DM 96.00.

Described by Dom David Knowles as "without question the most familiar figure among the monastic writers between the Conquest and the Dissolution," (*The Religious Orders in England*, 1 [1962], p. 292), Matthew Paris is perhaps best known as the author of the *Chronica Majora*, a work written at St. Albans, and intended as a continuation of Roger of Wendover's *Flores Historiarum* from 1235 to 1259. In the present study Hans-Eberhard Hilpert focuses on a particular facet of the *Chronica Majora*: its transmission of imperial and papal letters and documents.

The strength of Hilpert's study rests both in its thorough understanding—and subsequent analysis—of problems inherent in Matthew's practice of historiographical method and in its willingness—not to mention its ability—to challenge and revise particularly knotty textual difficulties. In analyzing the place of Matthew in the tradition of Anglo-Norman historiography, Hilpert grapples with the issue of Matthew's questionable reputation vis-à-vis the use of documents. As Richard Vaughan notes in his *Matthew Paris* (1958)—still the definitive study for the full range of Matthew's career and writings—Matthew must be docked for "occasionally tampering with their texts, even to the point of deliberate falsification," and that Matthew "has something of the forger in him." Hilpert not only concurs but also states that he is not particularly concerned with the historical value of Matthew's sources, but rather

with how he uses them (pp. 20–21). Thus, from a careful study of variant readings in an exchange of letters between Pope Gregory IX and Germanus, Patriarch of Constantinople, (entered into the Red Book of the Exchequer and then into the *Chronica Majora*), Hilpert concludes that several derogatory interpolations concerning the pope, since they are not found in the Red Book, must be attributed to the pen of Matthew himself. Again, Hilpert's study of five letters from Frederick II to Earl Richard of Cornwall leads him to observe that Paris not only mangled the transcriptions of the letters but also adopted as his own the political opinions of Earl Richard. Regarding more specific points, Hilpert presents revisions of existing interpretations. Whereas Richard Kay had argued for 1234 as the date when Matthew took up with his continuation of Roger of Wendover's *Flores Historiarum*, ("Roger of Wendover's Last Annal," *English Historical Review*, 84 [1969]: 779–85), Hilpert proves that 1235 is the more likely date. Likewise, Hilpert overturns Vaughan's idea that Matthew consulted a *single* exemplar of the Red Book; a study of variant readings shows that the problem is more complicated than Vaughan had thought.

As these examples illustrate, Hilpert's study is valuable for the specialist interested in papal-imperial relations in the mid-thirteenth century, as well as for students of English cultural and administrative history. In the process of presenting the definitive analysis of Matthew's handling of all sorts of documents, Hilpert throws new light on the mechanics of, for example, the Exchequer, the transmission of correspondence, and even the practice of medieval diplomacy.

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STJEPAN ANTOLJAK. *Pacta ili Concordia od 1102. godine* [The Pact or Agreement of 1102]. (Sveučilište u Zagrebu, Centar za Povijesne Znanosti, Odjel za Hrvatsku Povijest, Monografije, number 9.) Zagreb: Sveučilišna Naklada Liber. 1980. Pp. 252.

Seeking conquest, King Koloman of Hungary marched on Croatia in 1102. History books state that representatives of twelve leading Croatian noble families met him on the Drava and recognized Koloman as their lord in exchange for retaining their positions in Croatia. Thus, Koloman gained Croatia by agreement.

This agreement, the so-called "pact" or "concordia" in Stjepan Antoljak's title, is, like most important early Croatian documents, controversial. The earliest text of the pact—and also the earliest reference to it—appears in an appendix to Thomas the

Archdeacon's *History of Split*, written in the mid-thirteenth century. This appendix, almost certainly not written by Thomas, appears only in certain manuscripts. The earliest of these is known as the Trogir manuscript. Recent scholarship dates it to 1387 or 1388; Antoljak plausibly redates it early fourteenth century. Even so, he still leaves it dated over 200 years after 1102.

Scholars have taken every conceivable position on the pact. These positions, summarized in Antoljak's opening chapter on historiography, are: the pact's text is authentic, copied from a lost original; its text was drawn from oral tradition or a lost chronicle whose contents accurately conveyed the agreement; its text was drawn from unreliable sources; a pact existed but the fourteenth-century copier changed its text to suit fourteenth-century reality or needs; a pact existed but the surviving text is not based on it but is a forgery to suit fourteenth-century needs; there never was a pact and its existence and text are simply forgeries. So, was there ever such a pact? If there was, does the surviving text accurately convey its contents?

Hungary did annex Croatia and the Hungarian king became a dual monarch crowned also king of Croatia. The Croatian nobles had great local autonomy and various defined privileges. These could have been based on such a pact; they could also have been achieved by other means.

Having discussed the manuscript tradition, Antoljak turns to the "pact's" contents. He argues that the twelve nobles could well have been Croatian leaders in 1102. The data he gives, however, show that all twelve named families had ties with Zadar and its environs. It seems odd that Croatia's fate should have been decided by a group drawn from one area; this might suggest that the text was forged in Zadar. Antoljak further argues that the contents fit the situation in 1102: since nothing is anachronistic they could well be from that time. He also notes that the contents, which describe the privileges the Croatian nobles actually had, are confirmed by subsequent history. This argument, of course, works both ways, for a forgery would also have been based on these subsequent conditions.

In the end, Antoljak concludes there was a pact: the Trogir manuscript copier of Thomas copied the text from the pact itself or from a now lost Hungarian chronicle. He thus believes that it is a good source whose contents can be used. Antoljak, author of a handbook on sources about the peoples of Yugoslavia (1978), is a respected specialist on sources. His study of the pact is impressive. Unfortunately, owing to the scarcity of sources, he cannot resolve the problem. The pact could well be authentic as he argues. But the possibility remains that the text is either a forgery or a genuine document whose contents were altered in the fourteenth-

century copying. Thus, the question remains open. We may expect the controversy to continue.

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MODERN EUROPE

S. D. CHAPMAN and S. CHASSAGNE. *European Textile Printers in the Eighteenth Century: A Study of Peel and Oberkampf*. Exeter, N.H.: Heinemann Educational Books. 1981. Pp. xii, 257. \$45.00.

The transformation of the textile industry since the mid-twentieth century has again focused attention on the importance of finishing for volatile fashion markets, and historians are now starting to explore the part played by these processes in the past. This account of the two firms that emerged as the leaders of the English and French calico-printing industries in the late eighteenth century, when patterned fabrics were the craze, shows what a central role printing firms played in the expansion of the textile industry and in the evolution of a fully mechanized system of factory production.

Through insurance inventories, S. D. Chapman skillfully traces the development of the Peel enterprises in Lancashire. Supplying a market of unprecedented size, "Parsley" Peel, a Blackburn chapman, built up an industrial empire employing thousands of workers by the end of the eighteenth century. The achievements of Christoff-Philipp Oberkampf of Jouay were no less remarkable. S. Chassagne describes how this master craftsman from Germany industrialized a rural valley near Paris, kept abreast of advances in technology, and produced three-quarters as many printed fabrics as the Peels in the 1790s. Only after 1805, when he could no longer obtain Indian "white goods," did Oberkampf venture into spinning and weaving to guarantee his cloth supply.

The rapid growth of these enterprises in the second half of the eighteenth century took the form of duplicating workshop space and employing more craftsmen. The Peels invested in numerous print works, warehouses, spinning mills, and cottages in southeastern Lancashire. Oberkampf centralized his operations at Jouay, where in 1793 he erected the largest factory to be found at that time in Britain or France. Both firms employed large amounts of working capital and labor, developed effective management structures, and utilized similar technology.

Several points emerge from this comparative study. French manufacturers did not, as is commonly supposed, produce superior designs in the first three quarters of the eighteenth century. In print-

ing technology Britain maintained a lead that culminated in the development of roller printing with engraved copper cylinders. But the major problem facing printers who produced fabrics for middle-class and lower-class customers was Indian competition. Oberkampf, who only supplied the French domestic market, relied on the fine quality of Jouay fabrics to dispose of his output. The Peels, who catered to a larger market at home and overseas, depended more on competitive pricing, and the extent of their markets probably enabled the Lancashire firm to undertake longer production runs. Market factors, in short, rather than differences in technical knowledge, capital investment, or managerial style, account for the superior productivity of the English firm. To meet the threat of Indian competition to Lancashire calico manufacturers, English printers began to produce yarn in fully mechanized mills. This significantly reduced their costs of production and enabled cheaper British calicoes to invade new markets overseas. Calico printing, the authors therefore conclude, "represents the missing link between proto-industrial and the modern industrial system in the textile industries" (p. 215).

This study is an important contribution to a neglected aspect of textile development. Although the evidence is not conclusive, the authors have succeeded in modifying or challenging existing interpretations at a number of points, and one hopes that comparative studies of other industries will be undertaken along similar lines.

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PRISCILLA ROBERTSON. *An Experience of Women: Pattern and Change in Nineteenth-Century Europe*. Appendix by STEVE HOCHSTADT. Philadelphia: Temple University Press. 1982. Pp. xii, 673. \$35.00.

Richly evocative sources and elegant writing characterize Priscilla Robertson's long study of upper-class women in nineteenth-century Europe. She uses individual biography effectively to compare the nations of England, France, Germany, and Italy. The topics examined are grouped under two rubrics: "Pattern," including girls' upbringing, choice of spouse, marriage, sexuality, and childbearing in "traditional" Europe; and "Breaking the Pattern," including the revision of law, ideologies of change, education, careers, and women's movements. Robertson has written an integrated monographic study that describes, on the one hand, women's lives and what people thought about them; and attempts, on the other hand, to analyze the origins of, and national variations in, the women's movement. The

first aspect is more successful in its execution than the second.

First, however, a warning to women's historians and social historians. There is little new material here. The sources are the published writings of men and women who commented on manners, women, and the family; letters; memoirs; autobiography; and biography. This material is displayed and discussed by country and topic, but with no systematic theoretical perspective or orientation except an underarticulated belief in progress. Further, Robertson has written in isolation from most contemporary scholarship. Numerous studies are not cited, for example: Lawrence Stone's and Edward Shorter's works on families, Theodore Zeldin's two-volume social history of France, Martha Vicinus's edited volumes on women in Victorian England, and Jean Quataert's and Marion Kaplan's recent works on German socialism and feminism. Not an article in *Signs*, *Feminist Studies*, or any French, American, or English journal of social history is cited. The result is opinionated and particular.

Robertson, whose intellectual approach is probably closest to a national character analysis, generalizes frequently from one or two examples. An Englishwoman, she notes (p. 139), would have two or three servants, though a German family of comparable status would get by with one. The French, she believes (p. 224), were more frank about sex; this frankness, and the "clear need for change," led to a more rapid fall in the age at marriage of young men in France. In Italy, "feminist ferment and industrial development" had changed matters to such an extent that by the 1880s hopes were raised among Italian girls "that their marriages would be better than their parents'" (p. 219). All these assertions cry out for documentation, which Robertson does not provide. The sections on Italy can serve as an example of the distortion inherent in her method. Much space is devoted, on the one hand, to the Italian custom of *cavalieri serventi* or *cicisbei*, a kind of male companion to upper-class women. She cannot give us any notion of the incidence of what she, and English observers, saw as a distasteful and bizarre custom. On the other hand, her examples of Italian women are drawn primarily from an unusually articulate but unrepresentative group, patriotic heroines of the Risorgimento. In the first instance, travelers' gossip and memoirs are the source; in the second, political history and women's own writing. These are interesting stories, but they tell us little about upper-class Italian women as a group. As for Italian feminists, Anna Maria Mozzoni has to do double service. Robertson's assertion that "Italian women . . . out of their disappointment with the Constitution and the Civil Code . . . formed a women's movement to demand for themselves the rights politicians had

kept from them" (p. 433) is completely unsupported.

Individual attitudes, consciousness, and action are central to Robertson's understanding. Even socialism is seen in very individual terms, as a kind of personal liberation ideology (p. 251), while the women's movement in England is seen (p. 523) as *sui generis*, incapable of structural or other systematic analysis. The interesting appendix by Steve Hochstadt on "Demography and Feminism" argues that nineteenth-century demographic changes, such as lower mortality and fertility in the upper classes and an increasingly narrow age gap between husbands and wives, provided a social-psychological background for feminism. Although suggestive, the data are not adequate to sustain his case. Neither personal consciousness or class-specific demographic factors can explain what is essentially a collective political movement. Thus, despite its descriptive richness, the study fails to make the connections it sets out to establish.

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ROLAND N. STROMBERG. *Redemption by War: The Intellectuals and 1914*. Lawrence: Regents Press of Kansas. 1982. Pp. vii, 250. \$22.50.

The relationship of ideas, action, and historical memory during the First World War has recently been the subject of some fine cultural histories: Robert Wohl's *The Generation of 1914*; Paul Fussell's *The Great War and Modern Memory*; and Eric J. Leed's *No Man's Land: Combat and Identity in World War One*. These works have attempted to distinguish the ideas and sentiments that provoked or were provoked by the war from the myths about the war that were constructed in a different postwar climate of opinion. Roland N. Stromberg's survey of the reaction of European intellectuals to the end of nearly forty-five years of peace is in this same general tradition.

Stromberg deplores the relative inattention scholars have given to the prewar intellectual movements that contributed to the war spirit and bemoans the tendency to ignore the conversions of intellectuals to their national causes in the "days of August" 1914. In a remarkably broad survey of prewar and wartime European intellectual life, Stromberg uses newspapers, books, memoirs, and propaganda tracts both to summarize the tendencies of intellectual activity in this period and to explain the direction it took. Stromberg is particularly successful in documenting the overwhelming unanimity, after some initial hesitations, with which intellectuals supported war efforts and in identifying the common

intellectual elements in that support across all national and ideological boundaries. Thus he is able to demonstrate the revolutionary, antibourgeois, and antimaterialistic sentiments that were shared by all European intellectuals and to reveal the high hopes that were attached to personal sacrifice, moral renewal, and a heightened sense of community. Stromberg is especially effective in reconstructing the atmosphere of solidarity that made alienated aesthetes and revolutionary socialists into dedicated chauvinists and that made pariahs of dissenters like Romain Rolland, who attempted to remain "above the battle."

He is somewhat less successful in explaining *why* European intellectuals displayed such surprising behavior because, on the whole, he accepts the reasons the intellectuals themselves offered for it. This approach makes it difficult for him to separate the institutional and nonideational influences that worked on intellectuals through their social and professional affiliations from those influences of a purely cultural variety. He therefore slights the common forces that shaped "high" and popular culture in similar ways. His bias in favor of the causal power of ideas leads him to advance the explanation that the war was essentially an explosion of fierce competitive instincts that were no longer as successfully sublimated as they had been in an earlier and less refined era. Devotion to this theory encourages Stromberg in some rather problematic speculations about the similar impulses motivating recent generations of radical youth, whose "violent" proclivities he believes issue from the same cultural contradictions.

These quibbles aside, Stromberg's panoramic survey of the nature and breadth of intellectual support for the war may give salutary pause to the "revisionist" thesis of a work such as Jean-Jacques Becker's *1914: Comment les français sont entrés dans la guerre*, which has tried to minimize the degree of emotional and ideological conviction that gripped the French at the outset of the war. Stromberg's study will be a useful starting place for further explorations of this important historiographic issue.

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DENISE ARTAUD. *La question des dettes interalliées et la reconstruction de l'Europe (1917-1929)*. In two volumes. Lille: Université de Lille III; distributed by Librairie Honoré Champion, Paris. 1978. Pp. xii, 999. 110 fr.

Until recently, many French diplomatic historians remained imprisoned within the linguistic confines of the hexagon. Even those who chose contemporary topics inclined toward the view, reminiscent of

the seventeenth century, that archives outside *la grande nation* could be safely ignored. The work of Denise Artaud and of fellow pioneers such as André Kaspi, J.-N. Jeanneney, Y.-H. Nouailhat, and Georges Soutou marks a welcome turning point in French historiography. The new generation is writing international history as richly documented and methodologically sophisticated as that produced by contemporaries in Germany, England, and the United States.

Artaud's scrupulously detailed *doctorat d'état* on the Allied war debts to America after World War I breaks new ground by exploiting government records and the major private collections in this country as well as the appropriate French archives. The material she has gathered will interest both American diplomatic historians and specialists on Europe. Her formidable scholarly apparatus, however, is pressed into service to refurbish the familiar interpretation that French nationalists have labored to make respectable for more than three generations.

The basis for Franco-American misunderstanding goes back to the first months of American intervention in the war. Washington made clear that European borrowers would have to repay their debts, if only at the preferential rate at which the Treasury itself borrowed from the American people. The Allies got the money at half the market rate; the funds could not have been raised on better terms. Yet the French government never took its obligation at face value. In 1917-18 the inspired Paris press focused its attention on the debt of gratitude that the United States owed France for bestowing on the world the principles of the French Revolution, while ignoring the monetary debt that France was incurring in America. The Commerce Ministry elaborated schemes for "pooling" raw materials after the war in the hope of consolidating an American subsidy for the French economy. Meanwhile, French military authorities sought to collect in cash for supplies furnished Pershing's army, and they actually charged rent for practice trenches behind the front placed at the disposal of the AEF. None of this fostered goodwill in the American heartland. But after the dust settled, the World War Foreign Debt Commission merely attempted to secure repayment of the postarmistice debt over sixty-two years, on terms reflecting French capacity to pay. Artaud chronicles the debt negotiations as part of the larger problem of European reconstruction, up to France's reluctant ratification of the Mellon-Béranger agreement in 1929. From the vehemence with which she writes, one suspects that in equilibrating the scales of justice she would still desire to levy a charge for the trenches.

Artaud arraigns the United States for a variety of high crimes and misdemeanors. She admits that actual war-debt payments remained small—in most

years less than Americans spent abroad on holiday. But psychology, she contends, proved more important than actual figures. The debts constituted the "stamp of American power on Europe," and for that reason Washington refused to follow the dictates of economic rationality. Artaud believes that failure to cancel in 1919 prevented a reasonable reparations settlement, retarded monetary stabilization, prolonged uncertainty in the international economy, and made capital flows erratic. She endorses a French proposal for \$6.5 billion in German reparations and mutual debt cancellation among the Allies, and she blames the United States for not having facilitated this arrangement by allowing public sale of reparations obligations. Whitehall's decision to consolidate its debt in 1922 reinforced American intransigence, she insists, and led to the Ruhr occupation. By seeking to divide its debtors, the United States prevented the reconciliation of Europeans.

In Artaud's eyes, every American action betokened iniquity. Washington's obduracy deterred Wall Street from lending a sufficient sum on private account to spur European reconstruction in 1919–23, and thus obliged European nations to depreciate their currencies. Thereafter, the U.S. government's insistence on debt collection required Wall Street to lend too much, thus transforming the European states into satellites. French leaders, she thinks, deluded themselves with a sentimental view of shared ideals and failed to grasp the imperialistic aims of American diplomacy. Only the Great Depression prevented the United States from manipulating the circular flow of capital to achieve the complete subservience of Europe.

Artaud adumbrates this thesis with technical skill and commands a lucid prose style. Yet she carries her interpretation to lengths that the documents cannot sustain and remains in some respects a prisoner of her passion. Her preoccupation with American wickedness leads her to overlook the great drama of the decade—namely the campaign by Great Britain, which clearly could pay, to embarrass the United States into cancellation without initiating a repudiation that would upset British capital markets. Artaud is right that Herbert Hoover saw the debts as a lever with which to intervene in Europe, but she incautiously attributes this view to others as well. Her claim that Eastern bankers employed the debts to make Europeans accept U.S. leadership of institutions such as the Bank for International Settlements flies in the face of the evidence. She underestimates the simple political pressure from the American public, especially in the West and South, for debt collection. The archival evidence suggests that policymakers from Harding to Hoover sought to follow a constructive policy on debts and reconstruction within broad limits set by public opinion. But Artaud recoils from that conclu-

sion and complains frequently about the "delicacy" of American documentation.

No one knows whether Calvin Coolidge actually said, "They hired the money, didn't they?" Yet one wonders whether some readers, upon finishing this indictment, will not murmur in exasperation, "se non è vero, è ben trovato."

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FRANÇOIS KERSAUDY. *Churchill and De Gaulle*. New York: Atheneum. 1982. Pp. 476. \$19.95.

This is a splendid book. François Kersaudy, a French scholar trained in Paris and Oxford, has chosen a fascinating theme: the story of the stormy relationship between Winston Churchill and Charles de Gaulle. In an excellent introduction Kersaudy explains that Churchill was a warm friend of France from his earliest years. De Gaulle's attitude to the British was less straightforward. He came from a class of Frenchmen who always suspected "perfidious Albion." De Gaulle was himself a difficult man to deal with. In 1934 he wrote a book in which he urged his countrymen to abandon their old-fashioned outlook in order to create an efficient and modern army based on crack armored forces. The book sold more copies in Germany than in France. When the disaster of 1940 fell upon the French, de Gaulle refused to submit to an armistice. He determined to continue the fight against the Germans and fled to Britain in order to do so. Churchill welcomed him with open arms, and he became, in his exile, a political as well as a military leader.

De Gaulle hoped to rally Syria and Lebanon to his side, and he was outraged to discover that the British maintained good relations with the Vichy leaders in those countries. He dismissed the British officers he met as "a fanatical group of British Arabophiles supported by the Colonial Office" (p. 144). In July 1941 he publicly declared that Britain was carrying on a wartime deal with Hitler in which Vichy served as a go-between. Churchill was outraged and a pattern in the relationship between the two men was now set. Whenever de Gaulle disagreed with British policy he issued incendiary statements to the press and severely berated any British officials who crossed his path. Churchill decided that he was an ingrate and an enemy of England. President Roosevelt's opinion of de Gaulle was another negative factor. He thought de Gaulle was an incompetent troublemaker and a fascist, and he regularly urged Churchill to control the Frenchman's excesses or suffer the loss of his own friendship. In time, Churchill became almost insane in his hatred of de Gaulle. He once sent a cable to Eden

and Attlee in which he said of de Gaulle that he was a "vain and even malignant man. . . . When we consider the absolutely vital interest which we have in preserving good relations with the United States, it seems to me most questionable that we should allow this marplot and mischief-maker to continue the harm he is doing" (p. 275).

Kersaudy possesses an outstanding narrative gift. His book is based on published works and on a great deal of unpublished material preserved in the archives of several countries, including Canada, France, Britain, the Netherlands, Norway, and the United States. He has exploited all these sources in the most admirable way.

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N. D. G. JAMES. *A History of English Forestry*. Oxford: Basil Blackwell. 1981. Pp. xii, 339.

The place of forests in English history continues to exert an undying fascination over the minds of a small but dedicated number of the historical community both professional and amateur. In the book under review, N. D. G. James, a former president of the Royal Forestry Society, has taken on the formidable task of covering the history of English forestry from 1066 to 1977. Any such attempt to provide a general overview of a complex subject over nine centuries will inevitably be open to criticism on matters of emphasis and uneven coverage of important topics. Whatever the failings of James's book in this regard, he is to be applauded for his effort and ambition in taking on this project.

The book's early chapters on the medieval forest law are sound enough in their grasp of basic facts and developments, although I do think that James, by concentrating almost exclusively on the royal forests as hunting preserves, underestimates their economic value to the crown. It is unfortunate that he does not seem to know of Charles Young's fine book, *The Royal Forests of Medieval England* (1979), or of Nellie Neilson's justly celebrated chapter on the forests in *The English Government at Work, 1327-1336* (1940). Awareness of such works would have reduced James's too ready reliance for the medieval period on John Manwood's *Treatise* of 1598 and his too easy acceptance of the medieval forest law and its administration as of a piece capable of illustration by examples drawn from any time between the twelfth and sixteenth centuries. In these early chapters James has a disconcerting habit of unexpectedly interrupting the flow of his narrative with what he seems to regard as quaint oddities. For example, Manwood's quite acceptable Elizabethan spelling of the word nuisance, "nusanee," is elevated to the

status of a technical term for an offence punished by the forest law (p. 24). One final comment on this early part of the book: James devotes chapters 4 and 5 to a listing of the names and boundaries of medieval forests. Such material would have been better off put in an appendix and the space devoted to the central concern of the book beginning in chapter 6, the "Development of Modern Forestry."

James is at his best discussing the exploitation of the forests in the seventeenth and eighteenth centuries for fiscal as well as for shipbuilding and other economic purposes. The section on forest management under James I and Charles I, however, would have been improved by fuller reference to the work of C. E. Hart on Dean Forest. The later chapters of the book, which take up the story of forestry from the mid-nineteenth century up to the 1970s, have too much ground to cover and become at times little more than catalogues of institutional developments and parliamentary statutes. Little opportunity is taken to stand back and assess the significance of the events being described. In the appendixes of what is intended to be a work of reference James provides an excellent glossary of old forest terms, but the oddly conceived bibliography does not provide the kind of comprehensive or even truly select list of works on forest history that would have been a real service to scholars.

What then is one's assessment of this handsomely produced and well-illustrated volume? For the general reader and nonspecialists in forest history it can be recommended as a generally accurate introduction to the subject, and it is to be welcomed as a useful addition to the literature on the Greenwood.

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WILLIAM W. MACDONALD. *The Making of an English Revolutionary: The Early Parliamentary Career of John Pym*. Rutherford: Fairleigh Dickinson University Press or Associated University Presses, London. 1982. Pp. 206.

There are a variety of ways to say that a book is unworthy of publication, but directness is probably best. This work on John Pym should never have come into print. In terms of the scholarship, it is a virtually unaltered version of a 1965 doctoral thesis. As such, there is no evidence that William W. MacDonald ever visited any archives outside the greater New York area. In addition, none of the secondary sources published in the nearly two decades since the research was done were used in the printed version.

These facts alone are condemnation enough, but the book is also filled with errors of fact that show

considerable inattention to scholarship. A partial list of errors includes naming Sir James Perrot as a privy councillor in the 1621 Parliament, claiming that the Lower Palatinate was safe from invasion until after that Parliament closed, describing Sir John Eliot as "both spokesman and champion of the rights of the House of Commons" in 1624, having Buckingham attack the Isle of Rhe in order to relieve a siege of La Rochelle, and creating Laud an archbishop by 1628.

The primary blame for this grossly flawed work lies with the editors of Fairleigh Dickinson University Press. Clearly they either did not have the manuscript judged by competent readers or they did not listen to what the readers said. Nonetheless, however slack the press was in fulfilling its obligations, the author should not escape without some responsibility. A person who, according to the dust-cover publicity, has published more than thirty articles, is the founder and editor of a journal, and has coauthored two textbooks, was not obliged to publish an outdated thesis in order to survive. Survival is the only possible excuse for bringing forth such useless work.

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LOIS G. SCHWOERER. *The Declaration of Rights, 1689*. Baltimore: Johns Hopkins University Press. 1981. Pp. xvi, 391. \$26.50.

Lois G. Schwoerer is to be congratulated for giving us the most thorough study of the Declaration of Rights that we have yet seen or are likely to see. Her book could become for the Declaration of Rights what McKechnie's became for the Magna Carta—the standard work. However, as much as she has done, she has not done as much as she thinks she did or wanted to do.

Schwoerer begins by stating her purpose. She intends to disprove the traditional interpretation, which said the Declaration of Rights and its statutory enactment, the Bill of Rights, were, in the words of Lawrence Stone, "a mere restatement of tradition, not an assertion of constitutional innovation." Schwoerer wants to show that the Glorious Revolution established a "new kingship," not just a "new king."

Her method was to take each draft of the Declaration and analyze each provision legally and historically. In addition she has examined the composition of each committee that worked on any single draft and has tried to determine which men were responsible for each provision. In this way she hoped to be able to determine the intent behind the words as well as to distinguish legal and constitutional effects

from the political and the propagandistic. Following this procedure, the middle portion of the book is a day-by-day, almost minute-by-minute account, committee by committee, provision by provision.

What is made most clear is that the original Heads of Grievances, which provided the basis for the first draft of the Declaration, was a combination of things. Old, undoubted rights were thrown together with claims to new rights that were a response to the actions of James II and Charles II. The Committee of Rights in the Convention recognized that some of its claims would require legislation. Others were put forward as being old rights but may not have been. The question of the illegality of the dispensing power is an example.

Schwoerer's conclusion is that the Declaration of Rights did provide for a new kingship and that it was largely the work of "radical" Whigs. William of Orange had agreed to far more than he would like to have because he was the prisoner of his own pre-invasion propaganda. He had promised to abide by the wishes of a freely elected parliament.

As thorough and as convincing as the book is, there is still something missing. In her zeal to show the radical nature of the Declaration, she slights the role played by the Tories. Those provisions dearest to the hearts of the Tories were placed first in the final draft. The process by which this was accomplished is not thoroughly discussed. The maneuvers of Whig leaders are always portrayed positively, while a motion of Falkland, the Tory spokesman, was said to have "concealed an ulterior Purpose" (p. 189). I think that Schwoerer's problem is that she has not given proper weight to the Anglican element in Toryism. Protestantism is treated too much as a whole. She does not clearly differentiate between Anglicanism and Dissent. An indication of her lack of concern for the Tories is her constant reference to the newly promoted Danby as "Duke of Carmarthen." He was only the Marquis of Carmarthen.

The Tories did support the Declaration in the end. Without Tory concurrence the Revolution of 1688–89 would have been neither glorious nor bloodless.

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SAMUEL PYEATT MENEFFEE. *Wives for Sale: An Ethnographic Study of British Popular Divorce*. New York: St. Martin's Press. 1981. Pp. ix, 336. \$25.00.

The most famous sale of a wife in British history occurs in a work of fiction. Samuel Pyeatt Menefee inevitably opens with *The Mayor of Casterbridge*, and he returns to the case at the end. In between he aims

to establish all that we know or ever will know about the sale of wives in Britain. He has identified nearly four hundred cases, ranging from the eleventh century until 1972, with the focus on the eighteenth and nineteenth centuries. It was in the 1740s that accounts of wife selling first began to appear in any number, with recorded cases being at their height in the classic period of the Industrial Revolution, 1785–1845. The decline in incidence started in the 1840s, before the 1857 Divorce Act, which is shown to have had little impact. Geographically, wife selling was more English than British and more urban than rural.

This is a work of scholarship; it is, if anything, over-burdened with footnotes, and there is an appendix listing all known cases and sources. It is also the work of an anthropologist. In fact Menefee aims to do what any historian would do, and that is to tease as much meaning as he can out of the information he has assiduously collected. The problem is that conclusions are hard to draw, and the author occasionally falls into the trap of linking cases without due regard to historical context.

The fundamental difficulty is that the survival of information about particular cases of wife selling may bear little relationship to the actual incidence of the practice. Nor is it easy to make sense of the cases that do exist; after considering the causes of wife selling, Menefee concludes in some exasperation that all “boil down to a single commonplace: the incompatibility of the wife and husband” (p. 66). Given marital breakdown, why resort to the public sale of the wife? Menefee convincingly places the practice in the context of a market economy. Wives were sold in the market or the inn, and the most apposite analogy for their sale was that of cattle; the symbolic halter made the comparison inevitable. While cattle, however, played a purely passive role in the auction, wives frequently agreed to the sale. Adultery by the wife was often a cause, and the sale was a public event to hand over the errant wife to a new husband and to relieve the first of any further responsibility.

Wife selling was an “informal institution”; that is to say, it existed independently of formal institutions such as church or state. It was appropriate in a society in which one form of control had broken down and another—that of the bureaucratic state—had yet to arise. Menefee sets it in the context of other informal institutions, such as jumping the broom or skimmington, and historians should welcome his attempt to rescue such practices from the realms of antiquarianism and fiction. If he is only partially successful in drawing conclusions, he has nevertheless written an absorbing book.

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PATRICIA B. CRADDOCK. *Young Edward Gibbon: Gentleman of Letters*. Baltimore: Johns Hopkins University Press. 1982. Pp. xvi, 380. \$25.00.

Patricia B. Craddock chooses as her subject not the mature historian of the *Decline and Fall* but the young scholar and gentleman whose intellectual development involved more ambiguities than the *Memoirs* admitted. By concentrating on the early life of a seminal historian, she has written a book similar in conception to John Clive's *Macaulay: the Shaping of the Historian* (1973). Both works reveal subjects of considerable complexity whose serene personalities in later life masked emotionally turbulent beginnings.

Craddock portrays Gibbon's childhood and early adolescence as a period filled with intense psychological stress. Although, contrary to his own published recollections, Gibbon was not one of a succession of brothers each christened Edward by parents accustomed to infant mortality, he did suffer in early life the death of his mother and a number of siblings. His conversion to Catholicism as a student at Oxford exacerbated a troubled relationship with his father, who banished him to the care of a Protestant tutor in Switzerland. Craddock traces with great diligence Gibbon's extraordinary self-education in Lausanne, where he mastered several languages and literatures. She defends his often caddish behavior toward Suzanne Curchod, arguing that his love of scholarship rather than the disapproval of his father deflected his passion.

It was, of course, on his pilgrimage to Italy in 1764 that Gibbon first entertained the idea of a great history, although Craddock shows that the famous scene “amidst the ruins of the Capitol” may have been a later fabrication of Gibbon's imagination. For a number of years after his Italian journey he found his desire to be a man of letters in conflict with his obligations as a country gentleman on an estate encumbered by debt. It was only after his father died in 1770 that Gibbon was financially and emotionally secure enough to begin the immense project that absorbed the remainder of his life and assured his lasting fame.

In its portrayal of Gibbon's character and the depth of its research, this book far surpasses its only real rival, the biography by D. M. Low published in 1937. Craddock is a meticulous scholar who has consulted all the relevant sources. Her analysis is sensitive and penetrating, especially in the opening chapters where she reconstructs the early life of her subject with sympathy and imagination. Yet, in an admirable attempt to be thorough, Craddock sometimes devotes far too much space to summarizing and quoting extensively material that contributes little to her story. This work spares few details. The

result is a biography where too often the art of narrative suffers from the zeal for comprehensiveness.

D. L. LEMAHIEU
Lake Forest College

J. E. COOKSON. *The Friends of Peace: Anti-War Liberalism in England, 1793–1815*. New York: Cambridge University Press. 1982. Pp. vi, 330. \$39.50.

The "Friends of Peace" of J. E. Cookson's study were in no sense a disciplined or coordinated political movement, and their opposition to William Pitt and to the war with France was neither complete nor consistent. In 1798 and again in 1803–05, their patriotism overrode their pacifism and led them to support the regime and the policies they had so vehemently opposed. Their political influence outside their own social and religious constituencies was never very great. Nevertheless, Cookson's book does demonstrate that their response to the prolonged crisis of the French Revolutionary and Napoleonic era was historically significant in the long run.

Cookson writes that the Friends of Peace "constituted a fellowship criss-crossed by the ties of family, religion, intellect, profession and business" (p. 2). In tracing the interactions of concerned intellectuals, journalists, Dissenting clergy and laymen, and northern merchants and manufacturers, the book is both informative and persuasive. It is organized more or less topically, and the chapters on "The Liberal Press" and "Yorkshire and Lancashire" are especially effective. The antiwar protests, Cookson argues, provided both ideas and techniques of organization and persuasion that would be utilized effectively in the reform campaigns of the 1820s and 1830s.

These later developments would be associated with the movement the nineteenth century called "liberalism," and Cookson assumes that the term can be usefully employed to describe the social and political program of the Friends of Peace as well. In the opening chapter, he examines the varied eighteenth-century connotations of the adjective "liberal." In the rest of the book he is less cautious, calling his actors liberals in order to distinguish them from Tories or Loyalists on the one hand and from the class-conscious radicals celebrated by E. P. Thompson and his successors on the other. The categorization leads to some distortion, since it implies a political and ideological coherence that the book itself demonstrates the antiwar movement did not possess.

The choice of a topical organization was dictated by the nature of the subject, but it leads to some repetition of details of the military progress of the

French. Similarly, the tangled relations of the Friends of Peace with the Foxite opposition might have been compressed and concentrated in one chapter.

The themes that more usefully run through the entire book are the growing political consciousness of Dissent and the anti-oligarchical solidarity of the manufacturers and businessmen of the north. "Antiwar protest," Cookson writes, "offers a splendid view of the emergent pluralism of British society, especially of the non-Anglicanism and industrialism which between them were providing the strongest propulsion" to antiwar agitation (p. 202). Here indeed Cookson's approach provides important insights into a crucial era in the making of modern England.

CLARKE GARRETT
Dickinson College

D. W. BEBBINGTON. *The Nonconformist Conscience: Chapel and Politics, 1870–1914*. Boston: George Allen and Unwin. 1982. Pp. x, 193.

This is a brief but highly useful account of the political engagement and influence of Nonconformist Christianity in late Victorian and Edwardian England. Eschewing any attempt at interpretive originality, D. W. Bebbington sets forth succinctly and in straightforward prose the main lineaments of the "Nonconformist conscience." He further offers persuasive evidence of the *political* importance of the Dissenting churches in the period 1870–1914, surveying their role and influence on social attitudes and policy on five important issues: disestablishment; social questions such as prostitution and venereal disease, urban housing, temperance, and gambling; Irish Home Rule; imperialism; and education. He concludes with suggestions as to why, by the time of the Great War, the political force of Nonconformity was spent.

The study is important because at no time since the Long Parliament did Nonconformists exert such political influence in England. It was an auspicious time for Dissent: the Reform Act of 1867 had opened up the parliamentary system sufficiently to encourage pressure from organized groups such as the middle-class chapelgoers but the Franchise Act of 1918 had not yet widened the electorate to include the mass of unskilled workers untouched by religion. By 1900 there were in England and Wales 1,763,000 members in the Nonconformist denominations, compared in the same year to 2,043,000 Easter communicants in the Church of England. Although it is true that the established church was growing more rapidly at the turn of the century, active Nonconformists outnumbered active Angli-

cans. Shortly thereafter, however, Nonconformist membership went into sharp decline and has continued on that course throughout this century.

Bebbington sees in the "Nonconformist conscience" a broad moral-political crusade characterized by certain shared features. The aim was essentially negative and condemnatory, calling for urgent, even immediate action against what was deemed evil. Compromise with wickedness was impossible, and, when it occurred, it brought reaction and a pulling back from political involvement. It was only in the 1870s and 1880s, according to Bebbington, that the distinctive *political* features of the Nonconformist conscience emerged, as it aligned itself ever more closely with Gladstone and the Liberal party in the conviction that the state should promote the moral welfare of its citizens. At the center of the story are the tension and struggle between moral idealism and the practical constraints of effective political action. This uneasy tension points both to the strength but also to the reason for the long-term political failure of the Nonconformist evangelical churches, not only in late Victorian England but also in the modern period on both sides of the Atlantic.

Most noteworthy is the author's treatment of the irresolution and ineffectiveness of the disestablishment movement as it neared the Liberal seat of power, the odd support for Irish Home Rule, and the change in Nonconformity from opposition to foreign intervention to support—ostensibly on humanitarian grounds—for the growing tide of patriotic imperialism.

Bebbington does not seek covert motives, such as new social status, for these Nonconformist actions, and it is here that others will fault his study. The author attributes the political withdrawal and failure of Nonconformity to its ephemeral emotional base, its negative policy (effective in opposition but not in governing), and the need to compromise, a secularizing move that drained the movement of its religious enthusiasm. This study does not probe in depth the religious springs of Dissenting social action, and the specialist will want a more thorough analysis of specific crusades, but it does offer an excellent overview with extensive valuable notes to the primary literature.

JAMES C. LIVINGSTON
College of William and Mary

DAVID GOODWAY. *London Chartism, 1838–1848*. New York: Cambridge University Press. 1982. Pp. xvii, 333. \$39.50.

It is perhaps surprising that until now there has been no full-length study published on the Chartist

movement in London. David Goodway's book goes a long way toward satisfying that need. In broad terms Goodway accepts the established picture of the course of London Chartism: the movement was disastrously weak in the early years, 1838–40, but developed into a vibrant force in the 1840s. Goodway maintains that the movement's early weakness in the nation's capital proved fatal to national Chartist hopes in 1839, probably the only point at which large-scale insurrection was a real possibility. The course of London Chartism moved against the national trend; the major challenge from London came in 1848, not 1839 or 1842.

Beyond the opening sections, which discuss the character of London Chartism and chart the course of events, this important study highlights at least three major themes. First, Goodway correctly places much emphasis on the relationship between the London trades and the Chartist movement. From detailed work he qualifies Iorwerth Prothero's thesis that London Chartism drew its support predominantly from the "lower" trades, that is from those trades most vulnerable to open competition, "slop" work, and the overexpansion of production and the labor force. While shoemakers, carpenters, tailors, and silk-weavers were indeed the occupational groups from which Chartism drew the largest number of its adherents, Goodway argues that the crisis of the London artisans was more generalized than this suggests. Chartism in fact drew support from a far broader spectrum of artisanal trades, including many "aristocrats." Secondly, and in accord with his periodization of London Chartism, Goodway provides a detailed analysis of the movement in 1848. He attacks the myth that the events of April 10, 1848, constituted a "fiasco." Although the government prevented the Chartists from marching on Westminster to present their demands *en masse*, Chartists did successfully convene a huge demonstration on Kennington Common. Goodway contends that if there was a "fiasco," it is rather to be found in the massive overreaction of the middle classes who were frightened by events across the Channel. There is also a lucid account of the insurrectionary plots of 1848, which represent the last flourishing of a tradition of metropolitan conspiratorial action dating from the 1790s.

Thirdly, Goodway pays special attention to the Chartist crowd as distinct from the organized or official movement. Goodway argues, somewhat abruptly, that "Chartism's tragic predicament" stemmed from its historical placement between spontaneous "pre-industrial" forms of protest and "industrial modes of action" (p. 125). Particularly important is Goodway's careful consideration of the forces of ruling-class authority that Chartists faced in London. The presence of a large and efficient metropolitan police force marked a fundamental

difference between London and the provinces during the early Chartist years.

This study is extremely well researched; the footnotes and bibliography provide a mine of information. The volume is not always as well integrated as one might hope, and the author might have recreated somewhat more fully the texture of the Chartist movement, particularly its cultural dimension. However, these are minor disappointments in a book that will be of interest to historians concerned with popular insurgency, the development of industrial capitalism, and the maintenance of order in nineteenth-century European society.

JAMES EPSTEIN
Duke University

STEPHEN KOSS. *The Rise and Fall of the Political Press in Britain: The Nineteenth Century*. Chapel Hill: University of North Carolina Press. 1981. Pp. viii, 455. \$29.00.

Richard Cobden observed in the 1860s that "grown-up men, in these busy times, read little else but newspapers" (p. 130). This perennial British fascination with journalism serves as a focal point for Stephen Koss in his comprehensive and absorbing study, *The Rise and Fall of the Political Press in Britain: The Nineteenth Century*. In this book, Koss trains his considerable abilities on the complex interrelationship between politics and the press, from the middle years of the nineteenth century to the beginning of the twentieth century. The result is a considerable increment to our knowledge of both spheres of activity. A second volume is to follow covering the period to 1951.

Koss's thesis is that after the repeal of the newspaper stamp duty in 1855, close links were established between a political press, centered primarily in London, and miscellaneous political leaders and organizations. These ties differed from the previous system of direct government subsidies to newspapers that has been so ably charted by Arthur Aspinall in his pioneering study, *Politics and the Press, c. 1780-1850*. Although newspapers now boasted increased circulations and greater freedom and profitability, their political bonds were, if anything, strengthened. Subtle, roundabout methods of influence were used by Lord Palmerston, Benjamin Disraeli, Lord Salisbury, Joseph Chamberlain, and others, while editors, overcoming the stigmata of social and political inferiority, became, in the words of W. T. Stead, occupiers of "the only true throne in England" (p. 210). As Koss makes clear, the balance between the two estates edged slowly back and forth, affected as it was by many factors, including the public appetite for political news and technological changes in journalism. By 1900, commercial

considerations were predominant in the press. Less space was given over to parliamentary events and more to matter judged to be entertaining. Yet, even then, as Koss states, "the difference was basically one of proportions rather than emphasis" (p. 431).

Although the book is skillfully written and buttressed by a wealth of evidence drawn from private correspondence and newspapers, it has some weaknesses. Koss's seeming indifference to political events outside Westminster makes it difficult for him to provide a balanced assessment of a phenomenon such as the "New Journalism," which drew upon a popular readership. The subordinate place that he assigns to provincial newspapers, by virtue of their distance from London and the precincts of Clubland, is open to challenge. And the chronological structure of the volume, though essential for a clear analysis of successive political changes, makes for some dull stretches as hundreds of minor journalists (and scores of newspapers) put in brief appearances only to be shunted off the stage in favor of star performers.

Yet despite these minor flaws, this is a book that any historian would be proud to have written. It is bold and sweeping in its objectives, immaculately researched, and a significant contribution to the literature on modern Britain, both because of the questions it raises and the many penetrating conclusions that it reaches.

JOEL H. WIENER
City College of New York

WALTER L. ARNSTEIN. *Protestant versus Catholic in Mid-Victorian England: Mr. Newdegate and the Nuns*. Columbia: University of Missouri Press. 1982. Pp. viii, 271. \$20.00.

This is an important study, appealingly presented. Walter L. Arnstein has set out to rescue the anti-Catholic factor in nineteenth-century English society and politics from the obscurity that has always enshrouded it. As always, he tells his story well, with style and humor, weaving it around the career of Charles Newdegate, that honest Tory squire who typified the opponents of Roman Catholicism and long led them.

Historians have been in turn embarrassed and bored by nineteenth-century antipopery. It was embarrassing to contemporaries and boring to succeeding generations, and got in the way of the onward and upward path of toleration, which, as every student of Western Civilization knows, for all intents and purposes triumphed in the late seventeenth century with John Locke. Outpourings of anti-Catholicism, like continuing prejudice against Dissenters and Jews, were most inconvenient, and

had to be explained away as vestigial, the province of the ignorant and retrograde.

Arnstein takes a different approach. His book, he tells us (p. 3), sets out "to test three hypotheses: first, that it is more fruitful to look upon the Victorian conflict between Protestantism and Catholicism as a separate chapter rather than as a mere footnote to studies of the Reformation; second, that to describe that conflict solely in negative terms—as No Popery or anti-Catholicism—is deceptive in that it ignores the manner in which Roman Catholics in Victorian Britain could be both ardent missionaries and militant claimants of legal privilege rather than the merely passive victims of discrimination or the silent recipients of whatever crumbs of toleration happened to have been tossed their way; and, third, that the liberalization of the law often unwittingly helped to inspire religious discord."

The main hypotheses are triumphantly vindicated. Popish titles, oaths, and nunneries, as Arnstein firmly reminds us, were closer to the hearts of most mid-Victorians than the more edifying issues on which we might prefer to concentrate our attention. Few would deny the militancy of O'Connell (though most historians of 1832 appear to remain unaware that his, rather than a Continental model, was the sort of revolution the Reformers feared). Yet, as Arnstein demonstrates, Manning in his own way was no less militant. The author is also right to stress that constant pressure for the liberalization of the law—for Dissenters and Jews as well as Catholics—again and again renewed and perpetuated religious conflict.

In my opinion, this third, correct, contention casts doubt on the validity of one of Arnstein's subhypotheses. He argues for the importance in creating opposition to Catholicism of the "Protestant evangelical revival that was fundamentally antagonistic to the tenets of Rome" (p. 3). Given the violent opposition to any suggestion of liberalization of the law in the eighteenth century, it would seem difficult to prove that the revival made Englishmen more Protestant. It made them more religious—but that is something else. What was new, as Arnstein himself points out, was the steady pressure for liberalization.

This, however, is only a minor caveat. Arnstein has proved his main points, and they are of the first importance to a proper understanding of Victorian England.

R. W. DAVIS
Washington University
St. Louis, Missouri

JOHN BUTT and IAN DONNACHIE. *Industrial Archaeology in the British Isles*. New York: Barnes and Noble. 1979. Pp. xii, 307. \$23.50.

The last ten years have seen the publication of a number of single-volume introductory works on the industrial archaeology of the British Isles. All have certain strengths, but equally all inevitably suffer from attempting to be reasonably comprehensive within limitations of space so severe as to mean that the task is impossible. *Industrial Archaeology of the British Isles* is no exception, attempting to cover in 307 pages a vast range of industries and artifacts, principles and problems. The fact that John Butt and Ian Donnachie paint industrial archaeology with a very broad brush, allowing no constrictions of time period or exclusions of topic and insisting on a genuinely broad coverage regionally, is laudable, but it makes their aim doubly difficult to achieve. It is therefore no surprise that this book is tantalizingly unsatisfying, hinting at much but showing very little that is fresh or different. Although it is well written and the authors' dedication to their subject is evident, much of the treatment of the various thematic topics is so cursory as to be of limited practical use, even to the newcomer to the subject toward whom it is oriented.

Industrial archaeology is still in relative terms a young, evolving field of study, conscious of its undefined, unresolved position between history and archaeology, inevitably seeking to justify its claim to academic status, and unsure of its boundaries and research techniques. This Butt and Donnachie recognize in their thoughtful introduction but do not attempt to resolve, arguing that definitions of scope, technique, and time period are unhelpful. Although in principle there is merit in this proposition and one would not wish to argue, to the contrary, that the subject should be rigorously defined and that definition strictly adhered to, it is difficult to see the way forward without some clear agreement of what the subject is about. Only time will tell whether industrial archaeology will become a fully fledged discipline, of equal status to "conventional" archaeology, or remain confined to a backwater of local and amateur study; one fears the latter.

Perhaps the greatest weakness of the bulk of industrial archaeological research has hitherto been in its recording techniques, as the authors rightly stress. This lack is the more obvious in contrast to the refined techniques of the mature and elitist conventional archaeology. Unfortunately, however, the chapter on sources and techniques in this book is so brief that much of the advice verges on the banal. The section on photographic techniques, for example, says nothing about camera format, film types, lenses required, lighting problems, or the demands of architectural photography. Greater detail in this chapter, at the expense of greater overall length or the omission of the short and patchy gazetteer, would have made for a much more useful volume. The ten chapters that follow give brief indications of

the sorts of features that have survived in each of the thematic groups of activity, their historical context, and the scope for field study. Chapters on "agriculture, processing and the rural crafts" and "social archaeology"—principally workers' housing—are included, as are the usual more obviously industrial topics. The photographs are in the main good and well reproduced, although they betray the authors' Scottish base, and the bibliography, while lengthy, contains nothing published after 1977. In sum, this volume can be recommended to those who know little about the subject and want a workmanlike introduction, but, in common with most single-volume surveys, it is too superficial to have a wider attraction.

JOHN W. KANEFSKY
National Coal Board
London, England

W. HAMISH FRASER. *The Coming of the Mass Market, 1850–1914*. Hamden, Conn.: Archon. 1981. Pp. x, 268. \$27.50.

W. Hamish Fraser sets out to examine "how the mass of the British people spent their money in the decades after 1850, when a substantial majority of them had more to spend than ever before, and how commerce and industry adjusted to deal with the increased demand" (p. ix). This is, clearly, an ambitious project and one that also promises to be complex, fascinating, and useful.

The book is divided into three clear-cut major sections. The first part deals with incomes and with patterns in consumption of food, shelter, clothing, and luxuries. The second part, called "the stimulation of demand," describes changes in systems of credit, retailing, and advertising. The last part, "demand satisfied," details changes in food consumption, clothing, furnishings, and entertainment, using, for the most part, a series of mini-histories on such topics as bread, boot and shoe manufacturing, music halls, and cycling. Although this structure does lend itself to good order and a tidy presentation of relevant factual materials, it also gives the work a somewhat disjointed and static quality. More importantly, it militates against the answering of some of the most interesting questions implied in the author's statement of purpose. Relevant parts, such as changes in demand, in retailing, and in the patterns of consumption are described, but there is little attempt to discuss how the parts worked together in the creation of the whole, the mass market. On occasion Fraser acts as if the complex processes and interactions that ultimately created the mass market were as clear cut and straightforward as his structure. For example, the interrelationships of advancing transport technology, the

creation of mass transit systems, and the growth of the suburbs are not examined in detail but oversimplified into "Transport had to be provided as the suburbs moved out" (p. 8).

Although some of the potted histories in the last section are pretty bland fare, the book is, for the most part, well written. Its subject matter is interesting, it flows well, and it is often entertaining. Given these virtues and the fact that it suggests an interesting new perspective on English social and economic history between 1850 and 1914, it should prove attractive and informative for those just beginning to make an acquaintance with the field. Specialists, too, will find something of interest, although they will also discover that there is no intrinsically new material or hypothesis and that, although the questions it raises are fascinating, it is ultimately curiously unsatisfying.

JANET ROEBUCK
University of New Mexico

DAVID JONES. *Crime, Protest, Community, and Police in Nineteenth-Century Britain*. Boston: Routledge and Kegan Paul. 1982. Pp. xi, 247. \$32.00.

This is a book of essays on aspects of crime and authority in nineteenth-century Britain by David Jones, a historian already well known for works on the social history of that period. There are seven essays. Chapters 2 and 3 take the rural themes of arson and poaching, chapters 4 and 6 study urban-industrial Merthyr Tydfil and Manchester, and chapter 5 covers London. Chapter 7 examines the vagrant and crime, and chapter 1 provides a historiographical discussion of this topic.

This book is a major contribution to the subject of crime. Chapter 1 is probably the best available discussion of the state of historical writing on this area, the problems posed by the evidence, and the possible approaches by historians. In the individual studies, Jones faithfully practices what he preaches. Chapters 2 and 3 add considerably to our knowledge of the conditions, feelings, and protest actions of that still-obscure figure, the Victorian agricultural laborer. Chapters 4, 5, and 6 offer information on urban and industrial crime and police. For Merthyr, Jones draws usefully on his command of the Welsh context; for London, he uses a previously unused source—the Metropolitan Police Crime Reports for 1831–92; and he provides the first proper historical study of crime in Manchester. He makes use of material on indictable committals, summary trials, police reports and statistics, and newspaper reports in a skillful mix that avoids undue reliance on any one of these sources. Chapter 7 is the best and most original of all, exploring sensitively the history of the nineteenth-century vagrant, using a wide range of

hitherto-unused (especially local) sources, and being properly skeptical of contemporary "respectable" statements about vagrants. It brings out well the great use made by authorities of the 1824 Vagrancy Act, "one of the most flexible, useful and criminal-making statutes of the century" (pp. 206–07).

I must confess, however, to some disappointment in chapters 2–6. Jones is not able to bring his studies of rural and urban crime to any decisive conclusion: they end rather lamely, with some rather vague unproven assertions in chapters 2 and 3. This may be the product of proper caution about what can be drawn from the evidence, but it also stems from reluctance to state a clear conceptual approach to the problem of crime and law enforcement. Jones's approach seems to vary. At times he espouses a "social protest" view of crime and criticizes historians who underplay this; at other times he does exactly what he criticizes others for doing and asserts a growing acceptance of the police and the law in the second half of the nineteenth century. He never tries to reconcile these two views, using them at different points, as and when convenient, and never explicitly setting out his own approach. This criticism, however, really expresses my own disappointment that he did not do more with the magnificent material he has here assembled; this remains a most valuable and important book.

DAVID PHILIPS
University of Melbourne

STEPHEN HUMPHRIES. *Hooligans or Rebels? An Oral History of Working-Class Childhood and Youth, 1889–1939*. Oxford: Basil Blackwell. 1981. Pp. viii, 279. \$19.95.

This book includes excerpts from oral history archives at the University of Essex, Manchester Polytechnic, and the Avon County Reference Library. The Essex collection was initiated by Paul Thompson and Thea Vigne and from it came Thompson's *The Edwardians* and *The Voice of the Past: Oral History*. The latter book gives a methodological discussion of oral histories that Stephen Humphries assumes without further examination. Most of the oral history speakers from the Manchester and Avon collections lived in the Manchester and Bristol areas during the period covered by the book and were interviewed during the late 1970s. Almost no personal documents are included that originated during the period covered, unless the book's photographs are included in that category. The Bristol archive was organized by Humphries himself and is also being distributed by tape to schools and via the BBC to the public. The ratio of the number of pages of Humphries's commentary to that of oral history quotations (of one-half to one page each—a skillful

editing job) is almost two to one, which casts doubt on the author's claim that this is a "history of working-class childhood and youth largely in the words of working-class people who themselves experienced it between 1889 and 1939" (p. 3).

Oral histories are good at turning up information ignored or suppressed by official records. This book is not a comprehensive view of its subject but brings to light material on classroom disobedience, disaffection from school work, "larking about," social crime, street-gang violence, rebellious sexual behavior, and resistance to authority in reformatories. Humphries claims to have discovered new evidence of strikes at over a hundred schools in Great Britain during 1889–1939. Oral histories are much less good at supporting generalizations, and the basis (even the extent) of some broad statements Humphries makes is not apparent.

Oral histories are also good at changing stereotypes about social groups. Humphries has successfully intended for the testimony given here to be eloquent evidence that working-class youth during 1889–1939 (and by implication today and in the future) were not all ignorant, immoral, and brutish—"hooligans." They are seen reacting aggressively but reasonably to persistent cruelty from middle-class teachers and other authorities, who apparently were acting on their belief in that stereotype. (Oral histories of those teachers and officials are not given here, however, so it is impossible to see the full structure at work, not to mention an oral history view of larger social and political institutions.) Unfortunately, it is unlikely that people holding this stereotype will be reached by this book, though perhaps they will be by the BBC broadcasts. Humphries also seeks to oppose criminological theories of working-class deprivation and depravity but, since he gives almost no direct analysis of criminological writings (no American criminologists are even referred to), these "theories" are uninformed, some are long since discredited, and they are themselves stereotypes.

Finally, oral histories are good at shifting the conceptual lens through which a subject is viewed. Humphries intends to "offer an alternative, class-based interpretation of the behavior of working-class youth, which will situate resistance within various class formations and relationships" (p. 2)—the "rebels" of the title. The concept of class, however, does not explicitly appear in any of the oral history quotations themselves. Socialist youth, for example, are not quoted, and Humphries even complains that some forms of resistance were "for the majority a short-term family solution rather than a long-term political solution to their problems" (p. 172; compare p. 208).

Humphries's frequently reiterated claim that working-class resistance to middle-class oppression

must be or "can to some extent be seen as nascent expressions of class-consciousness" (p. 24 and throughout) is not clearly explained. This claimed relationship may be (1) *metaphorical* or *potential*, leaving the literal meaning or the connection with actual affairs unspecified; (2) *circular*, merely an application of the definition that opposes working and middle classes, the criterion of selection of an object of study being repeated as an apparent interpretation; (3) *inferential*, without the steps being given in the argument from oral history evidence to class-based hypothesis; (4) *practical*, expressing a desire that by reading or hearing oral histories the working class will indeed see matters thus in the future; (5) or *false*, since the evidence given does not support the claim. Unfortunately, this preoccupation with "class" prevents Humphries from presenting more concrete and promising suggestions, such as his explanation for high rates of delinquency among thirteen-year-olds (p. 153).

The oral histories themselves exhibit people reacting to individual cases of cruelty, especially the use of a cane in schools, and local communities resisting the authoritarian imposition of alien control and values. Perhaps the latter should include, ironically, the imposition in this text of an apparently nonindigenous and monolithic interpretation. This book thus raises in a most acute way, though it does not itself discuss, questions concerning "political" uses of oral histories.

JAMES BENNETT
University of Illinois at Chicago

RICHARD M. KESNER. *Economic Control and Colonial Development: Crown Colony Financial Management in the Age of Joseph Chamberlain*. (Contributions in Comparative Colonial Studies, number 7.) Westport, Conn.: Greenwood Press. 1981. Pp. xvii, 305. \$35.00.

This is a highly useful book that accomplishes in good style the task clearly delineated in its introduction. It probes the role of government in the economic development of Britain's tropical and subtropical dependencies. Richard M. Kesner provides much statistical data on colonial revenues versus expenditures, on trade, and on loans during the 1880–1914 period for all of the crown colonies. The focus is, however, on Chamberlain's time as colonial secretary (1895–1903), and most examples are drawn from Ceylon, Cyprus, Gibraltar, West Africa, the West Indies, and the post-Boer war Transvaal and Orange River Colony.

Kesner looks at things from London rather than the colonies. His main sources are published statis-

tics (the limitations of which he explains), the voluminous archives of the Colonial Office and Treasury in the Public Record Office, the private papers of cabinet members and leading civil servants, and (uniquely) the records of the crown agents.

Kesner is most interested in how the imperial bureaucracy functioned in making policy and in the attitudes and ideas of the "official mind." Since British aid for colonial economic development involved a vote by Parliament or loans backed with imperial credit, the Treasury claimed the right to review budgets of colonies borrowing or needing aid as well as proposed development schemes and loans proposed by any colony or the Colonial Office. Thus the drama is found in the interaction of protagonists from these two departments and in what happened to Chamberlain's schemes to develop Britain's "undeveloped estates."

Chapters deal with the limited revenue base of colonies in a free-trade empire; with the need for borrowing by government to create necessary infrastructure; with subsidies to postal, cable, and shipping services; with the Colonial Loans Act (abortive) and the Colonial Stock Act of 1900 (significant); with the regulation of banking and currency; and with controls imposed for the protection of the colonials upon private developers of forest, agricultural, and mineral resources. The footnotes are a mine of information. There are excellent thumbnail sketches of the careers of 50 civil servants and an extensive bibliography embracing general works on imperialism and colonies.

Chamberlain's efforts, implemented by hard-headed civil servants, did create new opportunities for colonial borrowing and did improve rail and sea communications. Britain, nevertheless, remained parsimonious with development funds; colonies were supposed to pay their own way and borrow only if revenue would increase to repay the loans (West Indian rehabilitation grants were exceptional). Kesner condones this stinginess and accepts the increase of raw material exports as the chief measure (along with native welfare) of colonial economic growth. These were the standards of the contemporary "public mind." Being no theorist, Kesner argues only mildly with the proposition that metropolitan policy (however benign in intent) and the capitalist system caused colonial "underdevelopment." He does not look within any colony to analyze in detail what may have happened; he assumes that policies formulated in London were carried out. It is therefore in the breadth of data analyzed and in the richness of detail brought to bear upon familiar themes that this study makes its strong contribution.

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DAVID FRENCH. *British Economic and Strategic Planning, 1905–1915*. Boston: George Allen and Unwin. 1982. Pp. x, 191. \$25.00.

That World War I was the watershed between *then* and *now* is seldom disputed, and Great Britain is as good a case study as can be found. David French attempts to place in a new perspective the background and awkward passage of that ill-prepared nation through a series of failed policies to, finally, a viable method of organizing the state to fight the Great War. In many respects this brief study evaluates political economy as John Gooch weighed military planning in his excellent study, *The Plans of War* (1974).

Britain initially attempted to wage war by relying on "Business as Usual": it would sweep the seas of the enemy, blockade the Central Powers, and contribute small contingents of men and large amounts of money to its allies, who would actually fight the land battles. This strategy failed, French tells us, because of the massive size of the conflict, because the blockade did not bring the German economy to its knees, and because the power of the fleet was negligible in the absence of a great naval war. Furthermore, it failed because the Secretary of State for War, Lord Kitchener, pressed on the nation a second strategy of his own—that of raising a gigantic volunteer army.

With the awed consent of his Cabinet colleagues, Lord K. committed Britain to a policy of the "Nation in Arms." By relying on unrestricted voluntarism, removing hundreds of thousands of men from the productive sector of the economy, and forcing the state to feed, clothe, and equip them, he effectively killed "Business as Usual." Failure of this second strategy, however, was also assured, for the War Office was simply unable either to maintain such massive numbers or to supply them, nor did it know how to foster the national economy the volunteers had left behind.

This led, then, to the third strategy, "Total War," the organization for war of the human and material resources of the nation through the machinery of the state. This, of course, led to the ultimate supercession of Asquith by Lloyd George and the implementation of what was called at the time "war socialism." At the point when the third strategy emerges, in mid-1915, French leaves off, perhaps saving the success of "Total War" for a future monograph.

This is a creditable effort. It is a spare book; in some ways it is the rarest of the rare, a book too brief rather than too long. French's forays into revisionism, for example (he dismisses the "Freedom versus Control" thesis of A. J. P. Taylor and Alfred M. Gollin as too ideological an explanation of these disagreements), deserve more space. Likewise, his

conclusion that the Cabinet system itself was to blame for all the muddle—so far as it goes—does not seem so very original, but it might have had it been carried a bit farther. However, let us not carp: books are shorter today than they were, and perhaps it is a good thing. This is an interesting essay that broadens our perspective of the World War, and this reviewer, for one, looks forward to French continuing his examination in future volumes.

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KENNETH O. MORGAN. *Consensus and Disunity: The Lloyd George Coalition Government, 1918–1922*. New York: Clarendon Press of Oxford University Press. 1979. Pp. ix, 436. \$44.00.

This book is vintage Morgan. Like *Wales in British Politics* and *Keir Hardie*, it is meticulously researched, with a clear theme and with plenty of the usual asides announcing corrections in the work of lesser men. Morgan has provided not so much a history of the decline and fall of the Lloyd George coalition as an analysis of the causes of that dramatic process. Quite correctly he devotes much space to foreign policy, an area of activity that most of Lloyd George's biographers seem to be unaware their subject knew anything about. The hero, if there is one, is Christopher Addison, who was martyred for his attachment to the true principles of Liberal radicalism. Yet on the whole Lloyd George comes off very well. He attempted seriously to find a middle course between socialism and reaction, to push forward a program of social reform, to arrive at an accommodation with militant labor, and to induce France to allow Germany to return to the European family of nations.

The trouble is not that these statements are untrue but that they are not new, and Morgan goes to painful lengths, including the misquoting of other books, to try to prove that they are. He belabors endlessly the tired old Abrams-Titmuss argument on the correlation between public participation in war and the incidence of social reform afterwards. He sets up straw men and demolishes them. This is a game for graduate students, not for well-established professional historians.

Nonetheless, this is a fine book, packed with information, densely written, and not for the beginner. One could quarrel with a few of the conclusions on foreign affairs. Morgan may underestimate the magnitude of the British victory at the Washington Conference and does not make clear the connection in Lloyd George's mind between this successful excursion and the Genoa conference where he hoped to repeat his triumph by settling Europe. There are some minor errors on Ireland. But all in

all this does not detract from the worth of this study. As an illumination of the murky politics of Britain's most mysterious coalition it will be hard to beat.

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J. M. LEE and MARTIN PETTER. *The Colonial Office, War, and Development Policy: Organisation and the Planning of a Metropolitan Initiative, 1939–1945*. (University of London, Institute of Commonwealth Studies, Commonwealth Papers, number 22.) London: Maurice Temple Smith, for the Institute. 1982. Pp. 285, £18.95.

During the second half of the nineteenth century, Great Britain extended its hegemony over vast portions of the globe. Initially these overseas connections consisted of a series of informal treaties and trade agreements. As imperial annexation among the European powers reached a fevered pitch by the 1880s, however, a formal, more complex structure of crown colonies, protectorates, and "areas of interest" emerged. The acquisition of this so-called dependent empire on the part of Britain was neither methodical nor systematic. Furthermore, it was not until Joseph Chamberlain's tenure as Secretary of State for the Colonies (1895–1902) that the British government made any concerted effort to devise development policies affecting its dependencies.

The transformation of imperial thinking regarding financial management and colonial development during the Chamberlain years was dramatic. It took the Colonial Office some time to consolidate and institutionalize the initiatives of his regime. The Colonial Development Act of 1929 marked a fresh beginning in imperial thinking. Unfortunately, the vicissitudes of the Great Depression of the 1930s rendered the act useless, necessitating a new approach. While many forward-thinking scholars and colonial administrators, such as Lord Hailey and Keith Hancock, spoke out in support of political, social, and economic reforms, the Colonial Office offered nothing more substantive than a re-examination of imperial policies toward the funding of infrastructure projects in the colonies. The Second World War changed all of that. The events of those years propelled British leaders both to address the issues of colonial development in the context of total war and to consider the status of British dependencies as part of a larger postwar network of relationships.

In their book *The Colonial Office, War and Development Policy*, J. M. Lee and Martin Petter examine the evolution of what they refer to as the "metropolitan initiative." They maintain that "the management of

the war effort by the Colonial Office on behalf of the colonies had a distinct effect on metropolitan conceptions of what could be done from the 'centre,' and that this positive experience of the 'lessons of war' was a key element in all subsequent handling of the traditional constitutional patterns in central-local relations" (p. 16). These "lessons" included centralized planning of large-scale and complex projects, and social engineering. The Colonial Office came to view imperial problems in a generic rather than a geographically specific context. Office staff grew in numbers (from 440 in 1938 to 1,284 in 1948) and served in subject rather than geographical departments. In so doing, the Colonial Office drew upon specialists in areas, such as finance and trade, and relied less heavily upon the localized expertise of the "men on the spot."

Lee and Petter reconstruct the policy process for us so as to demonstrate how these bureaucratic changes came about and what if any impact they had on decisions made in London. If some readers find their analysis a bit pedantic, this is the unfortunate price that one must pay when grappling with the policy process. A biographical appendix of the major actors in the drama would have assisted considerably. More importantly, the authors' presentation would have benefited considerably from the inclusion of greater detail concerning the substance of policies under review and the implications of those policies for the colonies concerned. All too often, Lee and Petter lead us through a complex web of negotiations and memoranda exchanges without indicating the importance of the policy as it finally emerged from the negotiations. In short, the reader is left without a clear sense of the Colonial Office's accomplishments and their effects.

The volume is nevertheless useful for the light it sheds on imperial decision-making during the war. While the Colonial Office was not represented in the War Cabinet nor in some of the most important war councils, many Dominion (self-governing) colonies and the United States were often present during discussions about the eventual fate of the dependent empire. The key concepts of "regionalism" (viewing colonies in terms of their geographic proximity) and "partnership" (those progressive steps leading toward decolonization and "commonwealth" status) were actively promoted by the U.S. and Canada in particular. The Colonial Office found itself obliged to respond to these ideas in terms of practical policies. At times, the specialist groups within the Office could exploit the sympathies of Britain's allies to obtain metropolitan support for changes in direction, while at other times they were subject to forces beyond their control. Thanks to Lee and Petter we now have a better sense of the forces at work at the heart of the British empire in the immediate postwar period. The perspective is helpful in decipher-

ing the events leading to the dismantling of the crown colony system in the subsequent two decades.

RICHARD M. KESNER

F. W. Faxon Company, Inc.

BETTY D. VERNON. *Ellen Wilkinson, 1891–1947*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N. J. 1982. Pp. 254. \$33.00.

Betty D. Vernon undertook no easy task when she chose to write a biography of Ellen Wilkinson, socialist, feminist, and Minister of Education in Attlee's postwar Labour government. Historical research on Wilkinson's career was complicated by the destruction of her personal papers shortly after her death in 1947. Vernon compensates for the missing papers by using interviews with surviving family members and friends, as well as Wilkinson's contributions to the labor press during the 1920s and 1930s. The amount of leg work that went into this biography is impressive. Unfortunately, these primary source exigencies caused Vernon to produce a work that is chatty and anecdotal rather than analytical and critical.

The author's insufficient background reading further detracts from the book's quality. Vernon relies on dated, albeit classic, secondary sources for the labor and women's movements. She seems unaware that there are more recent books on industrial conditions and women's trade unionism than *The Town Labourer* by J. L. and B. Hammond and *Women in Trade Unions* by B. Drake. But, more important, Vernon's biography of Ellen Wilkinson is, ideologically speaking, twenty years old. Throughout, the author shows little awareness of feminist historical issues. For instance, Wilkinson was both a socialist and a feminist, a combination apt to be uncomfortable, and there is evidence to suggest that she was often troubled by the apparent need to choose between helping the men and women of the working class from which she rose and helping women of all social backgrounds. While in college Ellen met Mrs. Bruce Glasier, who admonished her, "We need young women for socialism, they are all going off asking for votes and forgetting the bigger things" (p. 21). Afterward, Ellen recalled walking home from the meeting with her male companions "excited and serious" but bitter, for she said to them, "Only you fellows will be able to get into parliament and do the job, and they won't even let me vote" (p. 21). Vernon frequently provides this sort of tantalizing material but neglects to use it to make a point about Wilkinson's consciousness and its development over time.

Nor is it only Wilkinson's feminist consciousness that remains unexamined; so do the subtleties of

her political philosophy. Vernon suggests that Wilkinson's views were shaped by her membership in the Fabian Society but never specifies precisely how. As a result, the reader becomes puzzled because the author also indicates that Wilkinson's socialism was too revolutionary for many Fabians. In addition, the author fails to notice the role cooperation played in shaping Wilkinson's socialist ideology, especially her disdain for international monopoly capitalism. This is a surprising omission as Vernon thoroughly discusses Wilkinson's appointment as organizer for the Amalgamated Union of Co-operative Employees and her bill to toughen hire purchase regulations, a *cause célèbre* of cooperators anxious to protect consumers. Overall, the high point of the book is the second last chapter, where Vernon does an excellent job of analyzing Wilkinson's developing views on education during her struggle to implement the 1944 Education Act in the face of Britain's postwar fiscal difficulties. If Vernon had employed this chapter's critical techniques earlier in the book, she would have produced an admirable study of Ellen Wilkinson and the ideas that motivated her.

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PATRICIA A. GAJDA. *Postscript to Victory: British Policy and the German-Polish Borderlands, 1919–1925*. Washington: University Press of America. 1982. Pp. xiii, 232. Cloth \$21.50, paper \$10.25.

I am astonished that any publisher could have seriously considered this book in its present form for publication. Patricia A. Gajda has gone through the Foreign Office and Cabinet papers on Danzig and Upper Silesia between 1919 and 1925 at the Public Record Office, Kew, and then reproduced them in this work one after another in more or less date order. No effort appears to have been made to spare the reader any of the documentation so assiduously collected. At the end of the work one is left in a state of absolute confusion as to what, precisely, British policy toward the German-Polish borderlands in this period was, beyond a general desire to disengage from the area coupled with an anxiety to increase its commerce. These are hardly novel conclusions.

The errors are legion. For example, one would hardly describe Sir William Tyrrell as "a legal advisor" (p. 168). Nor is the standard of English any better. Thus we have Sir Eyre Crowe, a master of the English language, putting "a clamp on all speculations" (p. 168), and we are informed that "the Admiralty would buy none of this" (p. 36). Ill-organized and badly written, this book should have been returned to the author for substantial amend-

ment and revision long before the publication stage was reached.

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D. GEORGE BOYCE. *Nationalism in Ireland*. Baltimore: Johns Hopkins University Press. 1982. Pp. 441. \$32.50.

TOM GARVIN. *The Evolution of Irish Nationalist Politics*. New York: Holmes and Meier. 1981. Pp. xii, 244. \$39.50.

These impressive books differ substantially both in method and use of source material. Tom Garvin often employs the apparatus, both conceptual and statistical, of political science. His most interesting sources are some surprisingly neglected police and state papers in the Dublin record offices. D. George Boyce, on the other hand, writes that his "book is mainly about Irishmen thinking aloud about their politics." It is based mainly on a remarkably wide range of printed and public sources, and there is avowedly no attempt at a statistical approach. Yet, despite these dissimilarities, both works are essentially general histories of Irish nationalism.

Given the existence of some good, though very different, books of this sort by F. S. L. Lyons (*Ireland since the Famine* [1971]), Lawrence J. McCaffrey (*The Irish Question* [1968]), and Oliver MacDonagh (*Ireland* [1968])—it might be asked whether these two new books are necessary. In fact, both Garvin and Boyce triumphantly justify their projects. Scholars such as Lyons, McCaffrey, and MacDonagh came to maturity during the relatively peaceful and moderate postwar epoch, which was rudely interrupted by the outbreak of the latest phase of Irish troubles in 1968. One effect appears to be that this earlier generation of scholars was not keenly interested in sectarian division in modern Irish politics. Garvin and Boyce, who have come to maturity as historians in a darker, more tense epoch, have no choice but to face the problems head on. Boyce offers his book as an attempt "to explain why Irish nationalist ideology has failed to realise one of its most persistent goals: the creation of a comprehensive Irish nation embracing all creeds and classes of Irishmen." Garvin argues that, despite the secular and liberal leanings of many Irish leaders and theoreticians, their followers were frequently sectarian and conservative in outlook.

Both books, for example, discuss the mass Catholic, nationalist, and sectarian Ancient Order of Hibernians, which enjoyed its heyday from 1906 to 1916, at much greater length than earlier general histories. Inevitably, perhaps with such long time

spans to cover, there are mistakes and some lapses in interpretative rigor. Boyce is a longstanding and shrewd critic of writers obsessed with what he calls the "charmed circle" of high politics. But the memoir and manuscript material of Irish politics is redolent not only of intrigues but also of the progress of the Irish revolution itself. Garvin, who does not miss a trick here, uses the recently released papers of two agrarian radicals, Michael Davitt and Joseph Connolly, to good effect. Boyce's work does rather neglect such useful sources.

Garvin, on the other hand, commits himself to a rather dubious analysis of the failure of constitutional parliamentary nationalism. He argues that the party became increasingly involved in imperial politics and increasingly out of touch with people at home. This point is linked to the claim that even before 1914 other groups were able to outflank it on the issue of agrarian radicalism. This argument is difficult to sustain on electoral or any other evidence. It seems much more likely that the party's commitment to agrarian radicalism, sometimes of a vacuous and sectarian variety, helped to foreclose possibilities of a historic compromise with Irish Protestants—possibilities essential to its long-term survival.

Both books are somewhat bleak and downbeat in tone. They are certainly—neither of them—"court" histories of Irish nationalism. It is to be hoped that they will have a considerable impact.

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MARTIN WALLACE. *British Government in Northern Ireland: From Devolution to Direct Rule*. North Pomfret, Vt.: David and Charles. 1982. Pp. 192. \$17.50.

Martin Wallace's new book shares the same strengths and weaknesses of his earlier book, *Northern Ireland: 50 Years of Self Government*, published in 1971. The new book, largely taking up where the other left off, is a straightforward narrative account, concentrating on events after 1968. The first chapter scampers through the history of Northern Ireland from its foundation until 1968 and is followed by two chapters describing the civil rights movement and the desperate and unsuccessful attempts of successive Unionist prime ministers to preserve regional government in the midst of ever mounting violence and increasing pressure from Britain. The rest of the book is devoted to British attempts after the fall of Stormont in 1972 to limit violence in the province while attempting to restore some form of devolution. One chapter looks at the short-lived power-sharing Executive of January–May 1974, while four chapters deal with the twists and turns of direct rule from Westminster, which Wallace divides

into four phases, largely according to changes of governments in Britain. The penultimate chapter of just over fifteen pages swiftly dispenses with such aspects of government as the economy, employment policy, economic and physical planning, "social initiatives," human rights, and judicial reform. An enigmatic conclusion asks "Had the 1920s no lesson for the 1980s?" (p. 181).

The book is not without merit, since it relates clearly many of the chief events of the 1970s and gives useful summaries of the policy statements and reports that proliferated in those years. Particularly helpful are the digests of and extended quotations from the various inquiries into the administration of justice in Northern Ireland and the treatment of prisoners. For example, "Bloody Sunday," January 30, 1972, when thirteen civilians were shot dead in Londonderry by troops of the First Battalion Parachute Regiment, is examined through the *Widgery Report* to answer such questions as "who fired first?"

The book's shortcomings are just as obvious, however. It is almost totally innocent of analysis and interpretation to help the reader through the confusion and violence so clearly chronicled. Despite the title, there is no probing of the nature of British interest in Northern Ireland or of British understanding (or lack of understanding) of its problems and politics. More needs to be said about the reasons why, despite the alarming experience of Stormont, successive British governments have been determined to foist some form of devolution on the six counties instead of devising a proper and well-integrated system of direct rule. Likewise, there is no attempt to examine the nature of political loyalties and political violence in Northern Ireland and relate them to British policy. The failure of such initiatives as power-sharing in 1974 are explicable only in terms of the political ideology of the Protestant community and its resentment at being, as the Unionist head of the Executive put it, "kicked around" over the preceding five years. Similarly, plans to smash what were seen as the psychopaths of the Irish Republican Army took too little account of both the nature of the IRA and its complex relationship with the Catholic community in the North. In sum, Wallace's new book adds little to an understanding of British policy in Northern Ireland.

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R. J. KNECHT. *Francis I*. New York: Cambridge University Press. 1982. Pp. xv, 480. \$49.50.

R. J. Knecht's book on Francis I has been long awaited by those familiar with his articles on the reign of the king of the French Renaissance. It was anticipated in 1981 by Jean Jacquart's *François 1^{er}*,

but the latter work, concentrating on social and economic matters, actually complements Knecht's account of the politics, diplomacy, and scholarly and artistic patronage of the time. The author insists that he is writing history of the old-fashioned kind, neither *Annaliste* history nor biography. The king is the focus of the particular chapters, which are skillfully organized both episodically and thematically. Despite the denial of biographic intent, a clear picture of Francis emerges. His willful and licentious ways are offset by intelligence, courage, and, according to Knecht, humanity.

There are several general themes. The king chose military adventure before peaceful consolidation, but his obsession with war is treated sympathetically and the tactics of such celebrated battles as Marignano and Pavia are expertly analyzed. Much is also said on the despotic temper of the reign. Francis is shown with "an uncompromisingly authoritarian disposition, bent on centralization" (p. 360). Discussion of the provincial governors, the local estates and parlements, and the success with which royal legislation was enforced leads to the conclusion that it is misconceived to describe French government in this period as "contractual" or "popular and consultative." Not only does Knecht differ from J. Russell Major on this subject, but he also makes a case for the importance of the fiscal reforms of Francis I, thereby weakening Martin Wolfe's claim that the essential changes had occurred in the previous century. While the reign marked the beginning of absolutism in some senses, Knecht admits the practical limitations of royal authority and points out that Francis left such problems for his successors as institutionalized venal office. The government of the Renaissance king differed from that of Louis XIV in important respects.

Knecht often revises received opinion after careful scrutiny of the sources. He elucidates the clash between Chancellor Duprat and the Parlement of Paris. He denies that the constable de Bourbon was cast in the role of Coriolanus and rebuts allegations of systematic persecution before the constable's treason. He reassesses the usual view that the Burgundians preferred French to imperial rule, and he refutes Karl Brandi's contention that the treaty of Cambrai was a humiliation for France. He challenges the opinion that the king's personal pique at the affair of the placards was responsible for increased persecution of heresy. It was the sacramentarian message in this incident that condemned French Protestantism to become a religion for rebels. Francis continued to discriminate between heresy and humanism and persevered in his diplomatic overtures to German Protestants. This is but a sampling of the refreshing clarity with which Knecht disposes of old clichés, especially in diplomatic history.

A book of this scope cannot be equally strong on all fronts. There is occasional superficiality in the treatment of ideas and of the social context and a readiness to give the king the benefit of the doubt when in conflict with his subjects. So much color has been attached to Francis over the centuries that Knecht, while refuting particular images, cannot resist the inclusion of anecdotal material. At the same time, the king is no longer to be seen in one of his traditional roles as *le roi chevalier*, patron of the Renaissance, royal lecher, faithless diplomat, or spoiled infant cradled between his mother and elder sister. Yet the more complex and judicious picture does not deny Francis his glamor. *Francis I* is a book as splendid as its subject.

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RICHARD BONNEY. *The King's Debts: Finance and Politics in France, 1589–1661*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xviii, 344. \$54.00.

Desperate for cash to finance wars, French monarchs turned to great financiers or others for loans but repudiated debts at will. Repudiation, accomplished indirectly in more recent times through double-digit inflation, was done then through such means as stopping payments of perpetual annuities (*rentes*) or through group fines imposed on financiers; the government simply presumed that all had committed fraud—one of them might even purchase from the crown the privilege of fining the others! These are but a few impressions from the bizarre world of royal finance portrayed in Richard Bonney's book. Based on wide research among primary (including archival) and secondary sources, the narrative is well organized and readable—politics, war, and finance viewed from the center of government. In addition, 40 pages of appendixes list royal receipts and expenditures, finance ministers, and much more.

Personalities and events as Bonney sees them may differ from textbook images. Henry IV left behind a pile of bad debts; his minister Sully increased indirect taxes considerably and once declared that he had been hired to expand revenues, not to do justice. In 1626–27 Richelieu was contemplating "far-reaching structural reforms" (p. 130), including stopping sales of offices. But the year 1630 was crucial: the king's decision to retain Richelieu meant a policy of no coexistence with the Habsburgs and, eventually, financial abuses of "disastrous proportions" (p. 158). Claude Bullion's finance ministry (1632–40) witnessed increased expenditures and flooding of the market with annuities, followed by

their depreciation and a quarter century of chaos. The battle of Rocroi in 1643 appears indecisive in a war "essentially a struggle for resources" (p. 189). After the Fronde there was much business as usual as Nicolas Fouquet, finance minister and chief lender to the crown, let other financiers recoup losses suffered in the state bankruptcy of 1648. Bonney believes that as a group these men, including Fouquet, were not as rich as we have imagined. Colbert saw to that: as minister from 1661, he declared another bankruptcy and fined some five hundred financiers.

The root of the trouble was the king's expensive foreign policy. Apart from that, Bonney cites maldistribution of taxes and dependence on short-term loans or sales of annuities or offices, both these assets being subject to market fluctuation; as annuities depreciated, less cash flowed into the treasury. No public bank emerged to "regularize" the debt. Misleading, however, is the idea that the tontine, "introduced in 1653 on a small scale," was potentially "disastrous" (p. 280). Lorenzo Tonti's 1653 plan—to pay 5 percent to all ages for a nonreturnable principal of 20,500,000 *livres* (almost one-fifth of that year's expenditures)—never took effect. On those terms a tontine would have been cheap for the government if harmful to the buyer. Not till 1689 did the crown issue a tontine. But this is minor, as is the occasional misprint or ambiguity; for example, the passage concerning "real rate of subsidy" is unclear (p. 172).

Bonney has made much sense of a complex subject. This is an impressive book.

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MAARTEN ULTEE. *The Abbey of St. Germain des Prés in the Seventeenth Century*. New Haven: Yale University Press. 1981. Pp. ix, 210. \$20.00.

"Superiors and scholars may make an order famous, but that order cannot be devout and exact in observance without the ordinary monks." With this guiding principle Maarten Ultee offers a study of the Abbey of Saint Germain des Prés in the seventeenth century. He uses the techniques of the *Annales* school to place the monks of the Congregation of St. Maur in general and the residents of Saint Germain des Prés in particular in the economic, social, and demographic setting of seventeenth-century France. He treats principally the questions of monastic personnel, the economic foundations of the abbey, and the contact of the monks with the wider Parisian society from which they were cloistered.

The Abbey of Saint Germain des Prés came under the control of the reformed Benedictine Congregation of St. Maur in the early seventeenth century, and the abbey became the center of a network of about one hundred eighty establishments throughout France. From a period of rapid growth early in its history, the monastic population stabilized around the two thousand figure from 1670 to 1760. Despite a wealth of graphs and charts, information about the social origins of the monks is scanty, and contemporary discussions of social backgrounds are unreliable or deliberately avoided. We must rely on an impressionistic belief that a majority of the monks came from bourgeois families from northern France and had probably been attracted to the order by its educational opportunities and the wishes of their families. At Saint Germain des Prés there were in 1695 about fifty monks in residence. The revenues of the abbey were extensive; landed properties brought in an annual income of between sixty and one hundred thousand *livres*. The monks had excellent credit—as did the church as a whole—and they found it easy to borrow money when needed on fairly advantageous terms. Their major expenses went for the upkeep of their property and physical plant and for food. Although denied meat by their rule, the monks consumed relatively little in the form of cereals; thus their diet by common standards was luxurious. It is also striking that a minuscule portion of their revenue went for alms. Most expenditures were earmarked for the physical, spiritual, and intellectual welfare of the community. The monks seem to have had a better chance than the general population of reaching middle age, but their chances of reaching extreme old age were less than the general population. Contradictory explanations are offered: perhaps the austerity of their lives of fasting and penance was responsible, but their luxurious diet may also have played a role.

Ultee's study is a competent work. It demonstrates an agile use of economic and demographic data. But it delivers much less than it promises. Perhaps it is a valuable exercise in the strengths and limitations of this kind of history. Actuarial data about the monks seem to lead nowhere. Some graphs are incorrectly labeled or say the obvious (pp. 9, 72). Some graphs are called charts (p. 96). Odd translations abound, as does repetitious padding. The author is at times too close to his material and expresses himself obscurely. What does his reference to the "correct Christian attitude towards the poor" mean (p. 126)? He mentions that a leading monk was born on the Feast of the Nativity of the Blessed Virgin; of what importance is that for us? Finally, he is sloppy in his discussion of the abbey's traditional role in the consecration of bishops. Bishops are consecrated by other bishops, not by an abbey.

Useful, at times interesting, but not outstanding is the necessary judgment on this volume.

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Riverside

JOHN MCMANNERS. *Death and the Enlightenment: Changing Attitudes to Death among Christians and Unbelievers in Eighteenth-Century France*. New York: Clarendon Press of Oxford University Press. 1981. Pp. vii, 619. \$29.95.

Drawing on his immense learning and familiarity with eighteenth-century sources and on his no less considerable literary talents, John McManners offers a new synthesis for the English-speaking reader on a subject that has fascinated French historians over the past decade. A series of essay-like chapters explores the whole sphere of death and its kingdom, from demography and medicine, to heaven and hell, the deathbed, burials, tombs, even executions and suicide. Like Philippe Ariès and Michel Vovelle and Robert Favre before him, McManners charts the signs of some fundamental changes in attitudes toward death during the eighteenth century, among which the fall of hell, the secularization of wills, the decline of the community celebration of death, and the emergence of a "religion of tombs" are perhaps most important. His explanation for these transformations is complex and subtle. He takes note of the significant rise in life expectancy over the century, but concludes that most Frenchmen themselves never really perceived the change—and that some (especially the Physiocrats) had an ideological interest for arguing the opposite. Nor, in the end, does he accept Vovelle's hypothesis of a "de-Christianization" prior to the Revolution. The changes in the clauses in wills can be attributed in part to a certain "laicization," but also (following Ariès) to a growing confidence that one's family would respect one's verbal wishes for funeral arrangements, and to the rise of a more personal religion whose adherents no longer desired to publicize their beliefs.

Indeed, in the face of the more commonly held views on eighteenth-century Catholicism, McManners argues that there was a growth in the "quality of religious belief" toward the "rediscovery of the Christian doctrine of Love," developing dialectically with the religious thought of the deists and the Rousseauists. But the author admits that such an assertion about the qualitative would be difficult to prove, "for the registers in which the statistics are recorded are not accessible to the terrestrial historian" (p. 443). And one wonders how he would reconcile this proposition with the other indications of religiosity that seem to closely parallel the Vovelle trends: the rapid decline in clerical recruitment, for

example, or the fall in the vitality and new foundations of pious confraternities. Ultimately, in the view of McManners, the eighteenth-century evolution in attitudes toward death must be related to a much larger complex of change, closely associated with the emergence of the "affective individualism" postulated by Lawrence Stone: a transformation affecting attitudes toward marriage, the family, children, women, and sexual relations. But the articulation and interrelationship of all these changes can be only briefly sketched in a concluding chapter.

It is an enormously literate and evocative study. After the sweeping schemas of Ariès and the more austere quantification of Vovelle, McManners seeks to revive and recreate the individual men and women of eighteenth-century France in their feelings and reflections on death and on life. And in this he admirably succeeds.

TIMOTHY TACKETT
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JOHN A. CAREY. *Judicial Reform in France before the Revolution of 1789*. (Harvard Historical Studies, number 99.) Cambridge: Harvard University Press. 1981. Pp. xii, 162. \$22.50.

The mathematician and philosopher, René Descartes, observed that truth derives from observation of the world and the ability to express that observation in clear and simple categories and language. As often applied, the Cartesian method may create explanations that sound logical, clear, and compelling, but that utterly ignore the multiplicity of factors contributing to a historical event or process.

John A. Carey applies a kind of Cartesian approach to his study of judicial reform in France from the sixteenth through the eighteenth century. He begins with a premise many would accept: that the failure of reform efforts on the part of the crown made the Revolution possible. He then observes that the reason for the failure of judicial reform lay in "incompatibility and friction" (p. 6) between two definitions of reform: "traditional reform" meaning the return of a thing or idea to its original shape, and "modern reform," meaning the radical change or alteration of a thing or idea with a view to improving or ameliorating it. Carey argues that both these impulses were present, intermingled, in the writings and projects of a small group of jurists who sought to improve the administration of justice in the centuries under consideration. He develops and documents well the presence of both definitions of reform.

The Anglo-American legal observer will object, however, that precisely the same kind of conceptual ambiguity permitted common law judges on the other side of the English Channel to redefine the

rights of Englishmen while claiming to change nothing. For the word "reform," substitute the word "precedent," and think of Magna Carta's freemen becoming a nation of citizens. Lord Coke's insistence upon ancient rights as a barrier to royal prerogative, or even of the changing meaning of the writ of *assumpsit*.

In the English experience the fruitful tension, the ambiguity, lay in the context of the law itself, whereas in the French it was lodged mostly in an array of courts and court personnel. In the centuries under study, French courts developed and reinforced a system in which offices were irremovable, heritable, and venal and in which jurisdictions became increasingly more complex and contradictory. The system was not designed to render justice but to make money for judges, court officials, and the crown, which reinforced and extended it by the constant multiplication and sale of offices. An inefficient revenue-producing device at best, it fell into decline in the eighteenth century as judicial offices lost value due to their increasing number and the existence of other, more attractive avenues for investment. If there was a fatal contradiction, it was between the goals of justice and revenue generation, not between two definitions of the meaning of reform. Had Maupeou or another reformer been more resolute, or more resolutely supported, the fate of the system and of the crown might have been different.

Carey's book is written for the relatively small group of individuals who are concerned with the judicial writings of L'Hospital, Domat, Coquille, Bodin, Saint-Pierre, d'Aguesseau, d'Argenson, and others or who wonder why the presidial court (an intermediate, appellate jurisdiction) did not acquire more judicial business. It includes some attractive illustrations and a substantial bibliography. It does not touch upon related topics, such as the efforts of Pothier and others to derive a uniform law of the kingdom out of the maze of *coutumes* and vaguely Roman systems, or the little-known movement of litigation from ecclesiastical jurisdictions into royal ones. Narrow in focus, it studies ideas and reform proposals largely apart from the broader context of society, economy, politics, and personality.

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GEORGE ARMSTRONG KELLY. *Victims, Authority, and Terror: The Parallel Deaths of d'Orleans, Custine, Bailly, and Malesherbes*. Chapel Hill: University of North Carolina Press. 1982. Pp. x, 393. \$28.00.

George Armstrong Kelly insists that his book is the first part of a study of Jacobinism, in the sense that it describes men whom the Jacobins could not toler-

ate. This seems an odd way to go about things, since by 1794 there was very little that they *could* tolerate. Toward the end of his book Kelly claims that the four men he studies typified various kinds of aristocracy. He seems now to be suggesting that the essential characteristic of Jacobinism was its rejection of any claim to special status based on birth or former membership of a privileged corporation. He had presumably finished his book before he could read Patrice Higonnet's *Class, Ideology and the Rights of Nobles during the French Revolution* (1981), which suggests that the Jacobins used the bogey of "aristocracy" as a tactical convenience.

Rather oddly, Kelly has chosen to study four men who are interesting because of their singularity rather than their conformity to any particular type. All of them were executed for alleged opposition to the Revolution rather than because of their birth or previous membership in an academy or sovereign court. Most of their peers survived the Revolution and many opponents of privilege perished. It is difficult to see where Kelly's argument is leading.

What he does do is to give us four excellent short biographies of interesting people. Orléans, a prince of the blood, was suspected of being the nominal head of an influential faction in the first years of the Revolution. Bailly was the first mayor of Paris in 1789. Kelly does not say much about this, but he gives us some useful information about Bailly's early life. Custine is perhaps the weakest member of the quartet, since he kept to his soldiering, did not aspire to any political role, and owed his downfall mainly to bad strategy and bad temper. Malesherbes spent the Revolution in retirement until he courageously volunteered to defend Louis XVI at his trial, but it was not for that that he was executed. Although his relevance to a study of Jacobinism is perhaps marginal at best, his career under the *ancien régime* makes an interesting subject in its own right.

Whatever else is happening in the field of French Revolutionary studies, there is certainly a growing refusal to begin them in 1787 or 1789 and to ignore what the future revolutionaries had been doing for the greater part of their lives. This is something that everyone should welcome, and by showing that taking the story back into the *ancien régime* is not merely necessary but enjoyable as well, Kelly has put us all in his debt. How far he has told us much about Jacobinism is another matter.

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PATRICK H. HUTTON. *The Cult of the Revolutionary Tradition: The Blanquists in French Politics, 1864–1893*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xv, 218. \$24.50.

In recent years, the attention of historians—notably Patrick H. Hutton and Jolyon Howorth—has focused on the disciples and self-styled followers of Auguste Blanqui. An avid revolutionary who never led a successful revolution and who spent over half his years (1805–1881) in prison, Blanqui became a legend in his lifetime. The Blanquists, representing many diverse elements, had one common denominator, as Hutton sees it: their success in melding the legend of Blanqui and the French revolutionary tradition, particularly the Hébertist strain. Now Hutton probes at book length the activities of the Blanquists in French politics. He limits the Blanquist movement to an existence of only three decades, from the coming together in 1864 of a small group of restless students in the Latin Quarter to the formalization of schism in Blanquist ranks by the exclusion of the antiparliamentary Blanquists, the majority, from the Socialist Union of 1893.

Hutton would be the first to say that the term "movement," in its usual connotation, is not reflective of his material in this book. The very nature of Blanqui's thought served as an obstacle to any broad-based organizational effort by his followers. Hence Hutton's study is principally a contribution to the history of ideas in nineteenth-century France.

This is a book of considerable complexity, mostly—I believe—owing to the key ideas in Blanqui's less-than-systematic philosophy and to the ways his various followers interpreted them. Moreover, Hutton faced the challenge of examining these interpretations and explaining the actions arising from them over a particularly turbulent span in French history: from the troubled days of the Second Empire to the collapse of political Boulangism under the Third Republic. Within it lay not only the Paris Commune but also the beginnings of Marxian socialism in France.

As Hutton reminds us, to the Blanquist revolutionary the heart of the matter was the primal revolutionary idea, fixed and unchanging. In the "revolutionary situation," under inspirational leadership, the masses would experience renewal of the revolutionary idea and rise successfully against all authority. The fact is that Blanquist thought was anachronistic from the outset. By the time of the latter-day Blanquists who figure in Hutton's book, far-reaching changes on the European scene compounded their problems in revolutionary ideology. The hopelessness of the old barricade-style radical political action was crushingly demonstrated in 1848 and even more forcefully in 1871. Some democratization of politics in Western European states was opening new vistas to the socially and economically disaffected. Finally, the idea of change as process, whether expressed as evolution or dialectics, was winning the day. In contrast Blanquism held to a backward-looking, static conception of revolution.

With sharp perception, Hutton analyzes the stages through which Blanquist thought and the later Blanquist movement developed in response to changing times and the concomitant quarreling on the French left. He summarizes these stages (p. 166) as going from radical atheism in the 1860s through Communard nationalism and Boulangist populism to vitalist determinism. Overarching the particular concerns of the Blanquists in any one of these stages, however, was their growing preoccupation with myth and ritual to keep alive their ideological revolutionary renewal. Inevitably Hutton is drawn into a consideration of the force of tradition in collective mentality, one of his stated major interests.

Thanks to Hutton the reader is able to observe the evolution of the Blanquists from a small, disciplined body of conspirators to a fringe group noted chiefly as being able to mobilize the masses without reference to any particular cause. This ability of leaders, wholly deficient in the "heroic stature" of Blanquist ideology, to generate mass emotion was the rather empty outcome of what Hutton terms, in one of his most interesting chapters, the "politics of anniversary remembrance." For the Blanquists of the 1890s, agitation had triumphed over both revolution and parliamentary participation. Hutton finds no continuing place among them for "protosocialists" such as Vaillant and others. He is primarily interested, as is consistent with his earlier work, in the roots of twentieth-century mass politics and the formation of a radical, protofascist right in France.

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JOHN HUBBEL WEISS. *The Making of Technological Man: The Social Origins of French Engineering Education*. Foreword by DAVID S. LANDES. Cambridge, Mass.: MIT Press. 1982. Pp. xviii, 377. \$30.00.

John Hubbel Weiss's "technological man" is of a very particular kind. He is defined not merely by his title of *ingénieur civil* (one that was virtually unknown before about 1820) but still more precisely as a product of the École Centrale des Arts et Manufactures in the first twenty years of its existence, from 1829 to 1848. As a private institution founded to train engineers for industry, Centrale was a *grande école* quite unlike Polytechnique or such older schools as Ponts et Chaussées or Mines, all of which primarily fed the state services. Indeed, its founders—Alphonse Lavallée, Théodore Olivier, and Jean-Baptiste Dumas—always paraded the distinctiveness of their institution as a virtue: the *Centraux* were day pupils, they wore no uniform, and they followed a curriculum designed to bridge the gap between artisanal practice and the high-

flown mathematical abstractions on which *Polytechniciens* were disparagingly said to be reared.

Weiss shows how the founders' ideal of a unified *science industrielle* dominated Centrale's early years. "Industrial science is one" was apparently Dumas's maxim, and it was repeated time and again in justifying rhetoric of every kind. In practice, since "industrial science" embraced physics, mechanics, descriptive geometry, and chemistry, the oneness was hard to sustain, the more so as those who were engaged to teach these diverse subjects seem often to have fallen back on rather conventional demarcations between disciplines. In his handling of the contrasts between ideals and reality, Weiss is subtle and persistent, digging beneath the veneer of official syllabuses to unearth what the pupils of Centrale actually imbibed, socially as well as intellectually. He shows pertinacity, too, in his investigation of social origins. Here, his analysis of over 700 personal files reveals that two-thirds of *Centraux* between 1830 and 1847 came from backgrounds that can broadly be described as *haut bourgeois* (a proportion only slightly lower than that for *Polytechniciens* in the same period).

Refreshingly, *The Making of Technological Man* goes far beyond the bounds of traditional institutional history to embrace such wider issues as social stratification, professionalization, and the still-obscure relations between Saint-Simonian thought and French liberalism after 1815. The result is a book that combines definitiveness in certain respects with a pleasingly open-ended character. There are two outstanding questions that particularly take my fancy. First, where did the Centrale's concept of industrial science spring from? What precisely does it owe to the work of Charles Dupin and others who seem to have been moving toward such a concept (in the wake of Monge) well before 1829? Secondly, how did industrialists perceive the value of *Centraux* as practitioners? Put another way, how did the process that Weiss studies from the perspective of Paris appear when viewed from, say, Rouen or Mulhouse?

ROBERT FOX
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ROBERT TOMBS. *The War against Paris, 1871*. New York: Cambridge University Press. 1981. Pp. xii, 256. Cloth \$44.50, paper \$14.95.

The saga of the Paris Commune of 1871 has been written from virtually every political, social, and religious point of view. Nevertheless, Robert Tombs now provides a fresh approach. He examines the events of March, April, and May strictly from the point of view of the French Army of Versailles. He is most thorough in his work and the volume is

extremely well documented. He has consulted, and made good use of, the archival material at Vincennes (the Archives Historiques de Guerre) and Paris (Archives de la Préfecture de Police; Ministère des Affaires Étrangères, Archives Diplomatiques; Archives de la Seine). He also depends heavily on the memoirs and correspondence of participants and eye witnesses and shows a familiarity with the voluminous secondary literature.

The principal question that the author seeks to answer is why the Army of Versailles, which fraternized with the Parisian population on March 18, returned to the city in the third week of May and not only put down the Commune in a week of heavy fighting but engaged in the most bloody repression (ten thousand to thirty thousand executions) of the Communards when the fighting had ended. In the process of explaining these events, he lays to rest several popular explanations: the bitterness of the fighting of *la Semaine Sanglante*, the revengeful attitude of the returning prisoners of war, and the eagerness of the men of the provinces to put down a Parisian insurrection. Tombs's approach to an understanding of these seemingly contradictory actions is to analyze the make-up, the actions, and the attitudes of the army from the end of the Franco-Prussian War to the end of May. He finds the morale of the rank and file and the attitudes of the junior officers to have been woefully inadequate to deal with the Parisians before March 18 (the day on which the army evacuated Paris) or to retake the city in the weeks following the evacuation. It was, thus, necessary for Marshal MacMahon, the new commander, backed by Thiers and the Versailles government, to completely reorganize the army. Discipline was restored, undesirables were transferred to the provinces or North Africa, the soldiers were isolated from the civilian population and propagandized, and the prisoners of war returning from Germany were formed into new corps so that the strength of the army was brought up to 110,000 men.

Tombs makes several very interesting points in the course of this study. He demonstrates that the Army of Versailles did not change suddenly from a mutinous rabble into a hardened instrument of repression. Rather it was over several months that the arguments against the Commune (order against disorder, nation against faction, liberty against tyranny) came to be generally accepted by the army, but even in May many were not enthusiastic. On the ferocity of the fighting in the last days, he writes: "It was the character of the fighting rather than its scale, and its imaginary rather than its real dangers, that increased the savagery of this . . . war" (p. 165). And, finally, he concludes that "*La Semaine Sanglante* was the work of the generals. . . . MacMahon must have been broadly aware of events. . . . The worst

atrocities were carried out on the orders of [generals] Cissey and Vinoy" (p. 186).

Portions of Tombs's work will be familiar to French scholars, and, in a few cases, one may even have the feeling of *déjà vu*. Nevertheless, it is a sound study of the French army in this difficult period and an excellent examination of responsibility for the mass executions of May. It is a welcome addition to the literature of the Paris Commune.

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KATHERINE AUSPITZ. *The Radical Bourgeoisie: The Ligue de l'Enseignement and the Origins of the Third Republic, 1866-1885*. New York: Cambridge University Press. 1982. Pp. x, 237. \$29.50.

Contrary to much contemporary opinion and most current scholarship, Katherine Auspitz's study treats the radicals of the post-1848 era both seriously and sympathetically. She argues that the radical movement deserves far better than contempt and neglect, for its goals were laudable and it was chiefly responsible for the Third Republic's comparative longevity. Had the radicals been more successful in destroying or winning over their enemies on the left and the right, the Republic might still be remembered more unequivocally as "Marianne" than as "La Gueuse." Yet neither political nor social consensus could be achieved in any final sense in France, and in a game of politics that tainted all the players, the radicals gained a reputation—unmerited according to Auspitz—for pettiness, demagoguery, and scandal. This book describes radicals who were generous in their aspirations and effective in their agitation for educational reform.

The task that the radicals set for themselves following 1848 was no less than to define, organize, and establish an alternative society to the one they knew, dominated as it still was by aristocratic, conservative, and clerical values. The author reviews a selection of writings (Michelet, Quinet, Macé, and others) that define the qualities of the new republican citizen: a "modern masculinity" should permit every man to become, "like the *sans-culotte*, a citizen, a worker, and *père de famille*" (p. 32). Literate and well informed, the citizen would not have to accept the tutelage of notable or priest: he was the heir of the Revolution, not of the *ancien régime*. The citizen's wife and daughters, however, rescued through laical education from the supposedly baneful influence of priests, remained subject to their husbands and to male leadership in general. Although Auspitz reviews the classic argument of Michelet against the church's hold on women and discusses the educational programs for women advanced by Duruy and

his followers, she does not weigh the degree to which radicals, like their opponents, saw women as essentially dependent beings. This is merely to suggest that the history of radicalism in the nineteenth century, to which this book contributes brilliantly, is a vast subject that invites further exploration.

This study's chief accomplishment is to describe the work of Jean Macé and his allies in the 1860s, the establishment of the *Ligue de l'Enseignement*, and the agitation of this organization for free public education. The author concentrates on the *Ligue's* activities in several départements (Bouches-du-Rhône, Gironde, Rhône, Seine-Maritime, and Yonne) whose archives she consulted. From 1866 to the passage of the Ferry laws, this ostensibly single-issue movement attracted support from elements of the upper as well as the petty bourgeoisie on behalf of a republican regime. Whereas the Third Republic came into being for other reasons, Auspitz contends that it endured in good part because it could count on a broadly based radical bourgeoisie already organized around the issue of popular education.

The cynical view of public education, namely that it was a device conceived by the ruling class merely to discipline the newly enfranchised, has been fashionable of late, and indeed there is evidence to support it. But I find the present study refreshing in that it shares the optimism of radicals that basic education for the masses would ultimately help them to achieve personal freedom. Their descendants, inspired by their example, would gradually pry open the gates to the *lycée* and make the *école primaire* less of a social ghetto.

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DOMINICK LACAPRA. *Madame Bovary on Trial*. Ithaca: Cornell University Press. 1982. Pp. 219. \$19.50.

A poignant paradox in literary history is Gustave Flaubert's personal involvement with Emma Bovary, the feckless heroine of his *Madame Bovary* (1857). However self-consciously distant from the narrative, Flaubert could not resist the lure of his creation. "Madame Bovary, c'est moi, d'après moi," he often told friends, much to their bemusement and that of many subsequent admirers of his apparent stylistic detachment from the text. This intricate relationship between author and work, and by implication, between social and literary worlds suggests one reason why the novel has been misconstrued so often: in many ways the work is neither a lamp nor a mirror.

Dominick LaCapra's *Madame Bovary on Trial* probes the textual sources of this interpretative

conundrum by turning to the famous legal proceedings against Flaubert and his publisher during the Second Empire. The work's remarkable position between literary tradition and experimentation made for the misreadings evident in the trial; both the prosecution and the defense distorted the novel's achievement in their literal analyses of Emma Bovary's (im)morality. But the truly subversive aspect of the work, LaCapra argues, is not its content but its style, one that undermines the very grounds of moral judgment in the novel and ultimately in middle-class society. Deconstructive scrutiny of the text reveals this "ideological" crime committed by Flaubert's implicit stylistic challenge to the social and political values of his day. Just as the text calls into question the literary claims of the narrative subject, it also casts into doubt the bourgeois norms of family and religion. Hence, the prosecution of *Madame Bovary* while other works far more salacious in content escaped public notice.

Unfortunately, this valuable insight derived from literary theory and criticism suffers badly from haste. In only 210 pages of awkwardly organized text, there are 82 extended quotations, 33 from secondary sources. Fully one-fourth of the book is commentary on other literary specialists, most of which could have been omitted without loss to the author's argument. On the other hand, with his limited primary documentation, the author overlooks the historical question of the trial itself. LaCapra sought no archival evidence on why the Interior Ministry singled out Flaubert's novel for prosecution—even the attorney Ernest Pinard's journal is slighted. No other literary trials, including that of Baudelaire's *Les Fleurs du mal* (1857), are examined closely for comparison with Flaubert's. Moreover, the book discusses literally the published trial transcript and so fails to consider the legal briefs as texts worthy in their own right of deconstructive analysis. LaCapra's assertion notwithstanding, the trial was no more typically a nineteenth-century reading of *Madame Bovary* than the commentary of Second Empire critics (which the author also could have discussed).

In short, LaCapra fails to examine fully the historical context of trial and novel, even though such consideration is essential to a proper account of the work's readings. It seems that the author's infatuation with the text and its recent critics was even greater than that of Flaubert with Emma Bovary.

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FLORIAN TENNSTEDT. *Sozialgeschichte der Sozialpolitik in Deutschland: Vom 18. Jahrhundert bis zum Ersten*

Weltkrieg. (Kleine Vandenhoeck-Reihe, number 1472.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 240. DM 20.80.

In this useful, clearly written study, Florian Tennstedt surveys the history of state welfare activities in relation to lower-class social life from the mercantilist period through mid-nineteenth century social and economic liberalism to end with the social legislation of the 1880s and after. Eschewing theoretical constructs and polemics, he concentrates on two questions: What justifications were used to support or oppose state intervention? What were the thoughts of and effects on the recipients of such activity? While well aware of the hardships associated with capitalist development during mid-century, he does not dwell on class conflict or with matters of working-class solidarity. Instead, he traces the process of social distancing by the middle classes and the breakdown of the eighteenth-century concept of the working poor into the poor as distinct from workers. The poor became either those who could not work or those who would not work; in either case they could be subjects of state and local regulation. Workers, on the other hand, should fend for themselves. Tennstedt discusses at length both the conditions and treatment of the poor and of workers in both cities and rural areas, though most of his references are to Prussia.

The three chapters detailing this transformation and destruction of the absolutist welfare state lead into the final and longest chapter on the emergence of social policy as imperial policy in the 1880s under Bismarck. He serves the reader well in placing the well-known legislation of this period in a large historical frame. Such legislation was controversial but acceptable because it could fit under the category of aid to the poor. During the same period legislation to protect labor was very limited. Tennstedt argues that the conservative goal of involving workers in the state, however, began to become effective before 1914.

Tennstedt's strength is in a comprehensive integration of extensive material and recent scholarship to his themes. This is a social history in the sense of the conditions of life and labor, for there is little reflection here of recent trends in family or demographic history. While he does give citations, the absence of page references and the lack of an index are regrettable. The bibliography, which lists only works cited, is broad, though surprisingly, Karl Born's *Staat und Sozialpolitik seit Bismarcks Sturz* (1957) is not cited, nor are any foreign-language publications.

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HANS WILHELM BURMEISTER. *Prince Philipp Eulenburg-Hertefeld, 1847–1921: His Influence on Kaiser Wilhelm II and His Role in the German Government, 1888–1902*. Wiesbaden: Franz Steiner. 1981. Pp. ix, 189. DM 46.

From the 1880s to 1907, Count Philipp zu Eulenburg-Hertefeld combined the careers of artist, diplomat, chancellor-maker, royal adviser, cosmopolitan, and confidant. As Kaiser Wilhelm II's most intimate friend, Eulenburg juggled the personnel and the political crises of the 1890s until the pieces fell into the pattern of personal rule that Eulenburg was sure would preserve the Prusso-German monarchy and its social base. To reach his ends, Eulenburg used manipulation, love, psychological insight, and epistolary magic. His creation, however, worked more to destroy the empire than to save it.

Little of the drama or paradox surrounding Eulenburg comes through in Hans Wilhelm Burmeister's slim biography. His interpretation echoes the historiography of the 1950s, which claimed that, despite Eulenburg's efforts, personal rule did not exist. Since then, other historians have challenged that interpretation, but Burmeister neither confronts their counterviews directly, nor does he advance new information to bolster the older argument. Among recent works, John C. G. Röhl's published version of Eulenburg's papers should have been especially valuable to Burmeister, not only in interpreting "personal rule," but also in evaluating evidence (John C. G. Röhl, ed. *Philipp Eulenburgs Politische Korrespondenz* [1976, 1979]). Although he cites Röhl, Burmeister does not appear to have used him. Burmeister's main source is the manuscript of Eulenburg's papers, which Eulenburg tendentiously edited and even falsified, as Röhl points out (Röhl, vol. 1, pp. 53–75). Unfortunately, Burmeister accepts the manuscript's authenticity uncritically. He therefore falls into a number of snares the count set for unwary historians, such as believing in an undoubtedly phony telegram concerning the Reinsurance Treaty (p. 53) or accepting as contemporary, notes and marginalia written years later in the heat of scandal and self-justification (p. 166 n. 72). (For the phony telegram, see Norman Rich's *Friedrich von Holstein: Politics and Diplomacy in the Era of Bismarck and Wilhelm II* [1965], vol. 1, pp. 311, 316 n. 1 and Röhl, vol. 1, p. 517 n. 4).

More serious still is Burmeister's habit of abstracting Eulenburg from his context. The reader receives little sense of Eulenburg as leader of the civilians fighting Wilhelm II's military advisers, of his position in the entourage as a whole, of his friendship network and its advance into the upper echelons of politics, or of Eulenburg's changing relationships with the major figures of the day, not the least of whom was the kaiser. Without this

context, Eulenburg's activities seem like disconnected reactions to events, rather than well-planned manipulations that, in the 1890s at least, caused events to happen.

Just as Burmeister diminishes Eulenburg's significance in creating the far-reaching institutional changes of the 1890s, he also flattens out Eulenburg's character. Faint trace remains of Eulenburg the charming raconteur or brilliant letter writer. Burmeister is at great pains to obliterate Eulenburg's distinguishing peculiarities: he whitewashes the count's antisemitism, denies he was a spiritualist, and refuses to judge if he was homosexual. This last omission is especially serious, since homoeroticism, if not overt homosexuality, was the cement that bound count to kaiser, and that therefore formed the basis of Eulenburg's political power.

By minimizing Eulenburg's political significance and denying those characteristics that Eulenburg prized as his "peculiar individuality," Burmeister leaves us a pale chronology that obscures what it should reveal: the meaning of a tangled, complex, colorful life.

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DAVID SCHOENBAUM. *Zabern 1913: Consensus Politics in Imperial Germany*. Boston: George Allen and Unwin. 1982. Pp. 197. \$25.00.

Taking issue with the view that the Zabern affair exposed the Second Reich's basic illiberalism and incapability of peaceful reform, David Schoenbaum portrays it as a "Janus-faced" (p. 181) episode revealing the Second Reich as stable and flexible as well as "authoritarian and belligerent" (p. 180). He points out that public outrage assured that Zabern was an isolated incident and that the major military culprits were either transferred or punished for their high-handed actions. At pains to combat the school of thought that carps about the flaws of Wilhelmine Germany, he stresses that the Zabern affair cannot be used to demonstrate that the regime was any more "repressive than other Western societies" (p. 184).

Comparing German arrangements to similar ones in other countries, Schoenbaum traces the familiar history of the interrelationship between German society and the imperial constitution, concluding that the regime harbored so many contradictory tendencies that perceptible change was difficult. One of the changes difficult to perceive was the increasing meddling of the Reichstag in military matters despite the military's desire to avoid civilian scrutiny. Such meddling set the scene for the Reichstag's attempt to curb the military during the

Zabern affair. Yet even Schoenbaum cannot deny the constitutional duality that not only made the civilian and military authorities equal but gave the military precedence in certain situations.

Such a situation was the Zabern affair. Schoenbaum tells the story of the affair well. Comparing the incidents in Zabern to similar ones in other towns, where tensions were not allowed to escalate, he makes local military mishandling of the situation chiefly responsible for the affair. Yet the local commander, Colonel von Reuter, was strongly supported by General von Deimling, the commander of all the forces in Alsace-Lorraine and former conqueror of the Southwest African Hereros. Deimling's position reflected the ambiguity of imperial society and government. Stressing that the military could not countenance ridicule, he nevertheless admitted that the military intervention in Zabern, to stop both ridicule and harassment, was illegal. Further, as criticism of the Zabern affair mounted in the press, in Alsace, in the Reichstag, both the imperial Department of Justice and the Prussian cabinet decided, though not very publicly, that the military regulation dating back to 1820, on which the intervention was based, had to be changed. Schoenbaum does not make clear, though he would like to, that the alteration *substantially* limited the military's future freedom of action; the entire section dealing with Zabern's aftermath lacks the pointed analysis that would make his case convincing. And even Schoenbaum cannot reinterpret the fact that during the affair, the emperor and the military relegated the chancellor and the civilian governor of Alsace-Lorraine, von Wedel, to the periphery of decision making.

Schoenbaum's work is reminiscent of so many gentlemanly discourses: on the one hand this, on the other hand that. Though it is true that the German military and society were slowly changing, even in the midst of the Zabern affair, it is also true that the prerogatives claimed by the military vis-à-vis the civilian authorities before, during, and after the Zabern affair reflected the basic arrangement that proved to be so fatal for Germany during and after World War I. This book should be read for the new light it sheds on the affair and for the reminder that the Second Reich, like all societies, was a ramshackle, ambiguous structure. It can also be read as a warning against having too much complacency about those ambiguities.

If this book has one serious flaw, it is Schoenbaum's indulgence in cute verbal formulations. Describing the Prussian Lower House as "that proverbial monument to universal, unequal manhood suffrage" (p. 149), or von Wedel's under-secretary as "already a dead duck, if not a lame one" (p. 151), causing resolutions sent to committee to emerge

"from the parliamentary bedchamber" (p. 155), or couching new military regulations "behind figleaves of familiar language" (p. 161) may sound colorful while completing a draft at midnight, but inserted into a flow of normal prose, they unpleasantly jolt the reader at midday.

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MANFRED SCHECK. *Zwischen Weltkrieg und Revolution: Zur Geschichte der Arbeiterbewegung in Württemberg, 1914–1920*. (Dissertationen zur Neueren Geschichte, number 10.) Cologne: Böhlau Verlag. 1981. Pp. 365. DM 64.

MARY NOLAN. *Social Democracy and Society: Working-class Radicalism in Düsseldorf, 1890–1920*. New York: Cambridge University Press. 1981. Pp. xi, 376. \$37.50.

The writing of German history seems condemned to search endlessly for the causes of "the German catastrophe." Only the official dispensation in the GDR can afford the luxury of declaring itself to be "the winner of history." After the fledgling attempts by the first post–World War II generation to come to terms with the historical opportunities and failures of the German labor movement, it soon became clear that the next step would have to consist in a large number of local and regional histories. Naturally, these studies have been shaped by the experiences of the next political generation. Mary Nolan identifies herself as a member of the New Left, who wants to learn not only from its own successes and failures but also from the "learning process" (a favorite phrase) of the radical German workers. Manfred Scheck takes a studied neutral stance, but he too responds to the greater interest, since the late sixties, in the German radical left's past and to the late President Heinemann's appeal to study the November Revolution in greater depth.

These two dissertations provide useful background information on the economic development of their areas and try to reconstruct the social and cultural composition of the radical and reformist workers, but they focus explicitly on the level of the political institutions. As an American historian confronted with the ascendancy of the "New History," Nolan feels it necessary to reject monocausal explanations and "the prevalent tendency to downplay politics, parties, and ideology" (p. 306), although she remains, of course, committed to write history "from below." Scheck also writes from below, but without her conviction. In contrast to his relentless narrative preoccupation, Nolan has a theoretical axe to grind and time and again hacks away at the theory of "negative integration." Her study of the

radical Düsseldorf case is partially a critique of my 1963 formulation, but I read it largely as a confirmation of that theory, which was, after all, a "structuralist" statement about the relation between a dominant political system and an opposition movement on the national level. The very split of the labor movement, first threatened, then actual, weakened the overall revolutionary potential. Nolan herself stresses repeatedly that the radicals were isolated and essentially negativistic, lacking a positive vision.

The distinctiveness of Düsseldorf lay in the fact that the local Social Democratic majority fought the powerful unholy trinity of political Catholicism, Prussian bureaucracy, and conservative big business—and often enough the national party organs and national majority sentiment. Scheck justifies his selection of Württemberg somewhat half-heartedly by virtue of its intermediary position between important and unimportant states, its intermediate level of industrialization, its gradual evolution toward a liberal monarchy, and a revolution that was midway between the civil war in Bavaria and the tame events in Baden. Both authors show in detail, if proof were needed, that there was organizational continuity from the radicalism in the last years before the war to that of the revolutionary period. There was an organizational split in Stuttgart, the capital of Württemberg, as early as December 1914, and on the state level as early as July 1915. The situation was similar in Düsseldorf; by 1917 it was a stronghold of the Independents (USPD). But in contrast to Düsseldorf, where a radical Workers' and Soldiers' Council took over in November 1918, Württemberg already had a new SPD-controlled government on November 9, which was joined by bourgeois politicians the very next day and committed to parliamentary democratization. In Düsseldorf the Communists and a radical militia seized local power for a few weeks in early 1919, destroying the ballots in the municipal elections, before the Free Corps ousted them. In Württemberg, where the government came close to successfully creating a loyal democratic military, the revolutionary period came to an end with the failure of a Communist uprising in the summer of 1920.

The two authors agree that the Social Democratic leadership did not respond creatively to the radicalization of a large part of the working class during and after the war and especially that it failed to recognize the potentialities of the industrial and political council movement for the advance and defense of political democratization. The split of the labor movement proved irreversible and greatly affected the fate of the Weimar Republic.

Scheck argues correctly that local and regional investigations are especially important for a period of revolution, when the old and new central govern-

ment is weak or nonexistent. But now that we have a number of such studies, it seems to me time again to try for a synthesis on the national level, or else there will be diminishing returns from the pursuit of isolated cases.

Nolan took the risk of dealing with another country and another language and pays a price for it. Thus, the reader is erroneously told that "the revolution from below broke out a few weeks after the armistice had been signed" and that the state of "Württemberg" (*sic*) was one of the "Catholic South German states" (pp. 271, 201). There are also too many mistakes in the German items of her bibliography. Scheck did not expose himself to such hazards and practically avoided the non-German literature, misspelling only one of his two English references.

GUENTHER ROTH
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VAPPU TALLGREN. *Hitler und die Helden: Heroismus und Weltanschauung*. Summary in English. (Annales Academiae Scientiarum Fennicae, Dissertationes Humanarum Litterarum, number 29.) Helsinki: Suomalainen Tiedekatemia. 1981. Pp. 278.

This unrevised dissertation is yet another probe into the intellectual origins of Nazism, but one that is conducted in a superficial and misleading way. Tallgren argues that hero worship—the veneration of historical figures and the cultivation of such "heroic" traits as courage, shrewdness, self-sacrifice, and the desire to change the world—was a central element in Hitler's world view. Tallgren adopts an overly simple model of historical causation: Hitler derived his notions of heroism from the books and pamphlets he read in his youth and then translated his ideas into action.

Most of the book is devoted to the intellectual influences that allegedly contributed most directly to the formation of Hitler's world view. To the familiar list of Wagner, Schönerer, Karl Lueger, and Lanz von Liebenfels, Tallgren adds Hitler's schoolbooks (from which he supposedly derived his admiration for the monumentalism of antiquity), Carlyle (whose *Frederick the Great* he is presumed to have read), Karl May, and even Clausewitz, to whom Hitler occasionally referred in his speeches. Tallgren concludes that Hitler deliberately patterned the autobiographical portions of *Mein Kampf* on traditional hero myths and that his ultimate aim was to supplant Christianity with a cult of heroism.

Hitler und die Helden displays the irritating tendency of discovering anew what is already well known and of claiming as significant what is in fact trite. The most serious flaw of this study, however, is the

author's failure to discuss the sociopolitical uses of the cult of heroism, the social groups to which it appealed, or the historical circumstances in which it emerged. This is a work in the now justifiably discredited genre of intellectual history in which ideas are treated in isolation without reference to the sociopolitical context in which they arose. There is no appreciation here of ideology as the outgrowth or rationalization of interests; rather, ideas are treated as disembodied, self-generating entities and intellectuals as the causal agents of history.

The most useful part of the book is the description of Hitler's notions of hierarchy and heroism (pp. 47–82), since in this section there is at least some suggestion of how these ideas relate to anti-Marxism, antisemitism, and other aspects of Nazi ideology. The effort to establish a direct lineage of Hitler's ideas, however, obscures the fact that neo-Romantic notions of heroism, genius, and the creative personality were in general circulation throughout the latter part of the nineteenth century and formed an important part of the Germanic ideology of the Wilhelmine empire. What needs to be discussed are the reasons for the growth and spread of these ideas. Tallgren might have done well to consult the exemplary treatment of the personality cult in Richard Hamann's and Jost Hermand's *Gründerzeit* (volume 1 of *Epochen deutscher Kultur von 1870 bis zur Gegenwart*), a work that is missing from Tallgren's bibliography.

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DAVID PIKE. *German Writers in Soviet Exile, 1933–1945*. Chapel Hill: University of North Carolina Press. 1982. Pp. xv, 448. \$29.50.

This book deals with a subject that has rarely been treated with much breadth or depth: the plight of those communist writers who fled Hitler's Germany for the safety of the Soviet Union, only to find themselves plagued by a host of new difficulties, many of them life-threatening, many of them ultimately fatal to large segments of the German literary community in exile.

Based on extensive research in a number of Soviet, Hungarian, East and West German archives, as well as on invaluable private memoirs, personal interviews, and published material virtually inaccessible outside the USSR, David Pike has written what can only be described as an extraordinary book that has no equal in any language.

The story he has chosen to tell has many facets. It begins in the 1920s with the twists and turns of Soviet literary and political policies that had a direct bearing on similar disputes within the German

Communist party—disputes that persisted in one form or another well into the 1940s. It continues into the Hitler period, when numerous famous and not-so-famous novelists, poets, actors, and journalists (along with ranking KPD functionaries) sought political refuge in the USSR, where they naively expected to put their talents to good use in the service of revolution. This goal was at first thwarted, or at least tempered, by the united-front tactics that—as official policy in the Soviet Union in the mid-30s—called for broad-based antifascist unity over class-based revolutionary upheaval. The goal was thwarted even further and more seriously by the mass purges of the 1936–39 period. These purges affected not only Russian nationals but also all the German émigrés who, as foreigners, were easily open to accusations of being Trotskyists or Gestapo agents. Most prominent German writers were employed by German-language newspapers or were members of the German section of the Soviet Writers Union, and thus were subject to formal investigations, arrests, or late-night “disappearances,” all of which happened with great frequency, thereby decimating the German exile community. By this time most writers were forced to subordinate their revolutionary principles to the simple tasks of staying alive. Usually they did this by altering their political or literary positions as rapidly as the party did (out of fear of getting too far ahead of Stalin, or falling too far behind), writing whatever the party dictated, or by becoming informants and denouncing their friends and enemies among the German émigrés.

The whole story as Pike tells it—continuing to the end of World War II when many German writers became Soviet propagandists, writing leaflets that were then dropped behind German lines—is one long, discreditable episode that few survived with honor intact. By interlacing his account with a surprising amount of detail on the experiences of the participants, Pike is able to convey the anxiety of the situation; at the same time, he reveals a great deal that has generally not been known about the exile years of individuals like Georg Lukács, Willi Bredel, Johannes R. Becher, Alfred Kurella, Hans Günter, Friedrich Wolf, and many others. Except for a few printing errors and Pike's occasional penchant for making definitive judgments on matters that remain open to alternative interpretations, this is a remarkable piece of work: skillfully written, well-documented, and thoroughly informative on a subject that has not often been discussed. In other hands this book might have become a pedestrian recital of literary disputes and party controversies. Pike, however, has managed to draw out the highest moral and political implications of the drama of Russian exile, and this without bathos and without

forcing issues. His book will no doubt be the best available on this topic for some time to come.

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HARM-HINRICH BRANDT. *Der österreichische Neoabsolutismus: Staatsfinanzen und Politik, 1848–1860*. In two volumes. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 15.) Göttingen: Vandenhoeck und Ruprecht. 1978. Pp. xvi, 589; x, 591–1193. DM 345.

The generation of historians after World War II found relatively less enthusiasm for the study of the Habsburg monarchy of the nineteenth century than had its predecessors. Although the Napoleonic period and the revolution of 1848–49 certainly did not escape notice, the Vormärz, the period of neoabsolutism, and the *fin de siècle* were usually passed over by historians of liberal inclinations who found not much to excite them in those periods. In the last few years, however, neglect has given way to renewed scholarly attention of admirable quality.

The present work is a fine example of the new scholarship and is very nearly the realization of a historian's ideal, which is something akin to squaring the circle for a geometer. Harm-Hinrich Brandt has written a comprehensive, detailed history of administration, politics, and economics, and he has laced it liberally with excellent tables and informative graphs, without ever allowing his prose to become ponderous or his account to become tedious. On the contrary, these are two very hefty tomes that could also serve as reference works, but they are nonetheless gracefully and engagingly written. The work is a *Habilitationsschrift* that gives one renewed enthusiasm for that institution in German and Austrian scholarship.

Brandt opens his study with a description of the financial system of Austria in the Vormärz, proceeds to the financial exigencies and crisis of the revolution, and then affords a political, social, and economic overview of the succeeding neoabsolutistic regime. He then examines in closer detail the policies of investment and credit as well as the changes in the economic structure of Austria in the postrevolutionary period. He closes the first volume with an examination of the fiscal reforms under the neoabsolutist government.

The second volume begins with a chapter on budgetary and monetary policies of the Austrian state and follows with a chapter on the problems of financing the Italian war of 1859. The last chapter treats the declining fortunes of neoabsolutism and

its eventual demise. There then follow seventy-five pages of very useful tables and charts (including several tables giving the values of currencies and other measures), a very extensive and exceedingly useful bibliography of sources and literature, a register of the tables that are found throughout the text, and, finally, a very thorough index to the work. There are no maps, but readers of this work are likely to be well enough acquainted with the area not to need maps.

We have been inclined to look upon periods of absolutism and authoritarianism as periods of dullness, rigidity, and failure of imagination that lack all forms of energy except that of repression. Brandt counters this inclination by pointing out, rightly, that Austrian neoabsolutism intended to be energetic and resourceful in creating material prosperity and encouraging progress, thereby pacifying society. Indeed, some of the residuum of the revolution was not too much for the neoabsolutistic regime to use in their own fashion and purpose. For example, the enhancement of central power of government is desirable to authoritarian regimes; therefore, parliamentarism, which had been the child of the revolution, was adopted by the new regime (with its own notion of form and substance, of course), because parliament was an institution that emphasized centralization, the focus of government in Vienna.

Brandt has very ably posed the dilemma of the neoabsolutist: to be absolutist a government has to be free of the influences or demands of its subjects, whether exalted or humble, and to be able to assert its authority over the entire *Gesamtstaat* if it is to assert its validity. But such ambitious enterprise requires funds and income to keep funds plentiful. If the Austrian government wanted to acquire income, it had either to demand sufficient taxes or seek agreement and cooperation from the subjects to supply the necessary fiscal income. But to depend upon cooperation denies the principle of absolutism, and, in Austria, would have almost certainly required decentralization rather than centralization of government. The alternative—to demand—would have required force in authority, which would have had to be backed by an army. But armies are very expensive, and the ability to meet expenses was what the government lacked. An inexpensive army by conscription and the use of reserve officers would have required localization of recruitment and control as well as dependence on cooperative class and national interests. Austrian neoabsolutism never found the solution to this plaguing puzzle, although the defeat of the revolution in 1848–49 had left the government in promising circumstances to assert itself.

Austria's economic backwardness in the second

half of the nineteenth century made it difficult for the government to keep up with its international competitors. At the same time, its position at home was very delicate inasmuch as the neoabsolutistic regime sought to bring social peace through economic well-being—and in Austria that meant, at the very least, a healthy agrarian society and high agricultural production. Brandt points out that the revolutionary emancipation of the peasant, and the neoabsolutist following through on that reform, was a most significant phenomenon of the postrevolutionary period. Nevertheless, agriculture did not prosper sufficiently, and certainly industrial growth was too slow to afford a growth in fiscal income in such proportions as the government required to support its activities.

Although the government lived beyond its means and could never get its economic house in order, it did try to encourage private investment and employed governmental investment to try to invigorate the economy and bring general prosperity and well-being. It was not a period of stagnation, but rather was a period of energetic activity. But the premise of absolutism, that is, unwillingness—and possibly inability—to meet national or liberal aspirations as demonstrated in 1848–49, and the intention to use resolute and vigorous authority to govern, required what Austria did not have: uniform institutions, a uniform administration, and ample availability of resources to govern absolutely.

Brandt has given us a remarkably complete, well-balanced, well-written work that enables us to see clearly the components of the problem of neoabsolutism and the working of each upon the others as neoabsolutists tried in their way to master the art of governing an empire that would defy their efforts and those of their successors as well.

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MARTIN KITCHEN. *The Coming of Austrian Fascism*. London: Croom Helm or McGill-Queen's University Press, Montreal. 1980. Pp. 299. \$27.95.

This disjointed book is more likely to confuse than to inform readers who do not already know what happened in Austria between 1918 and 1934. Because Martin Kitchen treats events haphazardly and makes little effort to integrate material in his loosely connected topical chapters, the book lacks internal cohesion and chronological clarity. One comes away from it with the strong impression that Kitchen never decided what kind of book to write for whom. As it is, he gives us more a lament for the Austro-Marxists' theoretical and practical failures in the

face of an opposition determined to destroy their party than a careful study of that opposition, which the title leads the reader to expect. Unfortunately, he displays little understanding of the actual political relationships among Austria's several nonsocialist groups.

Surprisingly, the weakest chapter is the short one on "The Extreme Right." Although well versed in the various Marxist interpretations of fascism as a general phenomenon, Kitchen has not bothered to become intimately acquainted with Austria's particular fascists in whatever guise. He does not adequately describe or analyze either the Heimwehr or the Nazis, and these movements thus remain disembodied caricatures. Moreover, the reader also gets no clear sense of factors such as the crucial parliamentary role of the Heimatblock (which does not appear in the index) or the Heimwehr's "rolling putsch" in the provinces early in 1934.

The book is filled with annoying mistakes in syntax and facts. Not all early Heimwehr groups displayed the swastika, as Kitchen implies on page 55. The Heimwehr did not publish in November 1929 a program that "called for the creation of a fascist state" (p. 59); although the Heimwehr certainly took the lead in demanding antidemocratic changes in the constitution, the specific elements of those demands were first made public by the Landbund in October 1930. Starhemberg last met Hitler in April 1932, not sometime after Hitler became chancellor (p. 64). Kitchen's Minister of Interior Winckler (*sic*) and Vice Chancellor Winkler are the same person. Kitchen dates the Pfrimer putsch erroneously in March 1931 on page 104 and correctly in September in other places. The Nazi leader was Walter, not Alois, Riehl. After Hitler's accession was it "too late" (p. 265) or "almost too late" (p. 276) for Austria's socialists? Did Dollfuss entertain hopes of "defeating Austrian independence" (p. 283)? Finally, Kitchen omits some umlauts, all too many first names of Austria's fascists, and sufficient identification of primary documents.

Were the book not marred in so many ways, its positive features might be more readily appreciated. Kitchen scatters insightful generalizations about both the right and the left in Austria throughout the text. He argues effectively that socialist leaders should have supported Theodor Körner's conception of the Schutzbund as a guerrillalike force rooted in the working-class masses instead of Alexander Eifler's conception of it as a detached, regular military entity. A corollary is that at some (unspecified) moment well before February 1934 the party leadership should have drawn the line forcefully against their opponents. One can applaud Kitchen's pained and critical sympathy for Austria's democrats, as seen in his closing assertion that "their

struggle was for a decent, just and humane society" (p. 284).

In view of such valid concern, one wishes that Kitchen had constructed his book more painstakingly and that his editors had been more exacting.

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HANS B. KUNZ. *Weltrevolution und Völkerbund: Die schweizerische Aussenpolitik unter dem Eindruck der bolschewistischen Bedrohung, 1918–1923*. Bern: Stämpfli. 1981. Pp. 331. 65 FR.

When and why did Switzerland achieve the international respectability for which it became known in the West during the twentieth century? The question is particularly interesting because until World War I, Switzerland was widely viewed as an abode of liberals and radicals, a center of labor conflicts and socialism, and a haven for international agitators. Hans B. Kunz offers a persuasive, albeit partial, answer to the question in this study supervised by Jacques Freymond and inspired by the work of Arno Mayer.

Kunz suggests that a pivotal change in Swiss foreign policy occurred between the nation's general strike in November 1918 and the first meeting of the League of Nations in November 1920. In these two years Switzerland sought and gained *Salonfähigkeit* (respectability), becoming an active defender of the status quo instituted by the Western powers at the Paris Peace Conference. Kunz's central contribution is to show that the new course of Swiss foreign policy was powerfully influenced by the specter of Bolshevism.

Swiss authorities had reason to regard Bolshevism as a real and present danger late in 1918. They had just contained a violent general strike but failed to persuade the Allies of their nation's political reliability. Now they witnessed the spread of revolutionary disturbances in neighboring Germany and Austria. Such convulsions posed ideological dangers, disrupted the flow of vital imports (especially coal from Germany), and threatened to rupture other trade relations. Something needed to be done. Swiss diplomats supported the cause of Germany's "elements of order" in Allied councils, exerted pressure on Austria's socialists, and shipped food to starving Vienna—all in the hope of blunting the appeal of Bolshevism. In turbulent Latvia, Hungary, and Poland they worked assiduously to preserve Swiss and Western interests. Meanwhile, the Swiss government carefully monitored the activities of international socialist conferences, initiated political reforms that defused revolutionary agitation, and cooperated with the police of other nations to

combat communist operations. Special army contingents were stationed in frontier cities to prevent incursions by revolutionaries from abroad and to maintain order in the event of further disturbances at home. These and other measures helped to contain the dangers of Bolshevism and generated international esteem for the Swiss. The new *Salonfähigkeit* of Switzerland was affirmed in July 1920 when Woodrow Wilson designated Geneva as the seat of the League of Nations.

Kunz frankly bemoans the fact that anti-Bolshevik measures were necessary to achieve respectability. In effect, the Swiss had helped to avert revolution. But he concedes that most contemporaries (including Swiss socialists) probably sanctioned the government's antirevolutionary policy; and he observes pointedly that to condone revolution would have meant to restrict democracy. He is less convincing in the last section of the book. Here he traces certain compromising links between Swiss authorities and various counter-revolutionary groups. These connections lasted until 1923, but he fails to demonstrate their direct relevance to Swiss foreign policy. On the whole, however, Kunz's study is instructive and useful. He has strengthened the thesis that the "recasting of bourgeois Europe" after World War I was preceded by the containment of revolution.

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CLIFFORD W. MAAS. *The German Community in Renaissance Rome, 1378–1523*. Edited by PETER HERDE. (Römische Quartalschrift für Christliche Altertumskunde und Kirchengeschichte, number 39.) Freiburg: Herder. 1981. Pp. xvi, 208.

The last decade has witnessed increasing interest in the city of Rome in the Middle Ages and the Renaissance. Nevertheless, despite its great importance for students of these periods, the city remains substantially unknown. In particular, social historians have yet to undertake studies of population, occupation, wealth, status, family, and urban structures that would reveal much about the internal development of Rome. For this reason, this revised doctoral dissertation by the late Clifford W. Maas constitutes a valuable contribution to our knowledge, even as it suggests some of the problems to be encountered in the pursuit of solutions.

Choosing as his topic the German community in Rome, Maas studied the surviving records of German confraternities in Rome. The most important of these are the *Liber Confraternitatis* of Santa Maria de Anima, the records of the Campo Santo Theutonico, and those of the shoemakers' and

bakers' guilds. The nature of these sources dictated the main outline of his study. The most satisfying sections deal with German clergy attached to the Roman Curia, who played a leading role in the foundation and development of German national Church of the Anima, near the Piazza Navona. Materials for the studies of the laity are much less informative. German shoemakers played a significant role in the economic life of the city, though it is impossible to place them in the context of other, especially Italian, practitioners of the craft on the basis of the evidence presented. Maas has suggested that German bakers may well have dominated that business, but the evidence is much too sparse to make this more than a conjecture.

In general, the picture of German migration to Rome is one of rapid increase in the early fifteenth century, especially in the pontificate of Martin V and significant decline in the second half of the fifteenth and the early sixteenth centuries. Although Germans occupied important positions during the early period, they were increasingly displaced by Italians and came to play a lesser role during the second half of the fifteenth century. Despite this fact, Maas finds some evidence to revise our views regarding relations between such popes as Sixtus IV and the Germans. Still, he seems to lean toward the position that the Germans found Rome an increasingly less hospitable place. Despite the decreasing importance of the community, the Germans undertook major building programs in the late fifteenth and early sixteenth centuries. Much of the money to pay for these projects came from Germany through the sale of indulgences granted by the popes. Significant donations, especially for the Church of the Anima, came from a few wealthy curialists, most of whom were pluralists on a grand scale. Maas thus sees the Germans in Rome as contributors to some of the more well-known issues of the Reformation. Unfortunately, his study does not probe deeply the problems that led to these practices. To what degree did the experience of the German community reflect a set of economic and political problems confronting other parts of the Roman community and those of the contemporary papacy? What had happened to properties and incomes from investments during this period? Maas's explanation of the development of an exclusive clerical control of the confraternity of the Anima after 1450 would be more convincing if he were able to provide more evidence regarding the changes taking place in the lay community. His evidence excludes large segments of the laity from any consideration at all. Particularly unfortunate is the absence of material relating to the numerous group of German innkeepers in Rome.

Still, I concur with the decision to proceed with

posthumous publication of this work. Maas's critique of the sources makes a significant contribution to their understanding. He has been able to chart the chronological patterns and rhythms in the changing memberships of the confraternities in spite of difficulties that would have deterred many scholars. His judgments are careful and reflect his reading of the evidence. Though he has left questions unanswered that he might well have dealt with had he lived, he has provided a valuable foundation for future research and encouragement to all who are interested in the society of Renaissance Rome.

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MACGREGOR KNOX. *Mussolini Unleashed, 1939–1941: Politics and Strategy in Fascist Italy's Last War*. New York: Cambridge University Press. 1982. Pp. xii, 385. \$29.50.

This is a thoroughly admirable military and diplomatic history of Fascist Italy during the first two years of World War II. Started as a dissertation at Yale University, it has since been broadened by further research in archives in Rome, London, and Washington. MacGregor Knox's tightly written monograph consists of a preface, introduction, and half a dozen chapters that explore with a sharply critical eye the premises and limitations of Mussolini's foreign policy in the pre-1939 era, Italy's role as a "bellicose nonbelligerent" (September 1939–March 1940), the Duce's impatience to become "unleashed" (March–June 1940), the military actions in the shadow of Operation "Sea Lion" (June–September 1940), the attack on Greece (October), and Italy's end as a great power (winter of 1940–41). A concluding essay, "The Meaning of Fascist Italy's Last War," and 85 pages of footnotes, appendixes, and bibliography complete the book. Although the author's stern judgments may displease some Italian readers, Knox's meticulous study is likely to become the standard work on this period of Mussolini's foreign policy.

Because Fascist Italy failed the test of war—the only test its founder recognized as valid—many historians, in Knox's judgment, "have tended to underestimate [the regime's] brutality, the vigor and extent of its expansionist ambition, and the degree of domestic support its aims enjoyed until their price became fully apparent" (p. 1). The reasons for this are numerous, he suggests. Professional historians are often inclined to be naive with respect to the degree of unwisdom that prevails in the world of action. Moreover, certain Italian liberal writers like Benedetto Croce tended to dismiss Fascism as "antihistorical." Renzo De Felice, the principal Italian biographer of Mussolini, has indulged in a

certain measure of "apologetic nationalism" in painting the duce as a fundamentally humane dictator. Gaetano Salvemini and Denis Mack Smith have "exchanged analysis for sarcasm" in portraying Mussolini as an opportunist whose sole driving force was egotism and self-justification. Although Italian Marxists, in Knox's opinion, have been less guilty in underestimating Mussolini, they have erred in seeing the Fascist regime only as a "class dictatorship of the bourgeoisie," or perhaps a "reactionary mass regime." The regime was more than that, as Mussolini's goals and policies from 1936 to 1941 demonstrate. It is the author's contention that the growing power of a resurgent Germany in those years gave Italy unprecedented leverage and freedom of action.

Mussolini had a genuine foreign policy program, Knox insists. It was the creation of vital space for Italy in the Mediterranean and Middle East through force of arms. The duce viewed the policy of a balance between the Western powers and Germany with contempt. Mussolini's expansionism and his decision to go to war in 1940 proceeded above all from the dictator's own vision, not from internal social or political pressures and certainly not from "social imperialism." It was Hitler's successful blitzkrieg in May–June 1940 that enabled Mussolini to "unleash" his own long-contemplated imperial program.

Mussolini looked upon his war as "parallel" to that of Hitler's. He envisioned a short war, but hope of a swift naval decision evaporated as early as July 9, 1940, when Admiral Domenico Cavagnari's ships retired in confusion from a brief encounter with the British fleet. Subsequently, Mussolini proved unable to compel his admirals to seek decisive action. Meanwhile, on land, General Rodolfo Graziani revealed himself to be "an unparalleled virtuoso of procrastination." Stung by the German occupation of Romania, Mussolini ordered the invasion of Greece in October 1940. The ensuing fiasco in the Albanian mountains was the greatest blow to the regime's prestige since Guadalajara in 1937, and the greatest shock to its internal stability since the Giacomo Matteotti crisis of 1924. The German successes in the Balkans and North Africa in April and May 1941 stabilized the military situation and temporarily checked the dissolution of Mussolini's power, but the seeds had been sown for the coup d'état that the king and Marshal Pietro Badoglio would carry out against Mussolini in July 1943.

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NORMAN KOGAN. *A Political History of Postwar Italy: From the Old to the New Center-Left*. New York: Praeger. 1981. Pp. xviii, 177.

In 1966, Norman Kogan published volume 1 of his *Political History of Postwar Italy*. With this volume, which also emphasizes economic and political developments, Kogan continues his story through the years from 1965 to 1980. To my knowledge, this is the only book in English covering that period. It is well written, well organized, and, despite its brevity, contains a great deal of information and analysis. In addition to introductory and concluding material, there are two chapters on economics, four on political affairs, and one each on the trade unions and social change. Chapter 10, the longest by far, deals with both politics and economics from 1976 through 1980.

The chapters on economics are especially valuable, since readily available general literature on this subject for those years is very scarce. But the very best quality of the book lies in its truly remarkable "objectivity." Every important political party receives a fair shake in the sense that they each become historically "justified." A fine example of this is Kogan's chapter on the "Evolution of the Communists" during the 1960s. Characteristic is his assertion that "for the Communists, as for the other Italian parties, foreign policy was a tool of domestic politics" (p. 41). This approach permits Kogan to avoid a lot of misleading political rhetoric! Of course no one, including this reviewer, could be expected wholly to share Kogan's analysis of Italian communism. Here one small point should be made: since Kogan refers several times to the PCI as a "Marxist-Leninist" party (for example, pp. 34 and 130), he should also point out that the PCI, since its last congress, is now a "Marxist and Leninist" party. The decision to drop the hyphen was no small matter in the history of communism!

Very effective too is the way in which Kogan has constructed his book so that it revolves around the axis of events in the years between 1968 and 1970. As the author says, those years "mark a turning point in postwar Italian history" (p. 47). Though Kogan does not explicitly state it, he might perhaps agree that the long-term effects of 1968–69 were even more profound in Italian society than in French society. Yet informed Americans know a great deal about the latter and almost nothing about the former. With Kogan's book there is no longer an excuse for such ignorance.

As everyone knows, economic statistics are often unreliable. But, as Kogan says, they are probably more so in Italy. This is because the very large "illegal" sectors of the economy (*lavoro nero*) cannot be included. Therefore the Italian economy is considerably more active than official figures would indicate (p. 94); for similar reasons, the real rate of unemployment is much lower than reported—perhaps less than 2 percent! On the other hand, the author's report on crime statistics (p. 150) seems

more alarming than the reality, at least comparatively. Despite the activity of the Red Brigades and the well known crimes of passion, the number of murders per year is probably greater in New York City alone than in the whole of Italy.

I also question the value of the Gallup Poll of 1977 on "satisfaction with life" that Kogan analyzes (p. 158). This poll asserts that 69 percent of U.S. citizens were "highly satisfied with life as a whole," as were 41 percent of the West Germans and 26 percent of the French. Supposedly only 17 percent of the Italians were similarly optimistic, but Italians are notorious nay-sayers with regard to their own society and have arguably always been so, except perhaps during the Fascist "interlude." A few years ago, the veteran Communist Vittorio Vidali publicly denounced Italian nationalism as a "moribund turd." Perhaps there are representative political figures in other Western countries who have expressed themselves with analogous temerity and charm—but I doubt it!

Every reader of a book such as this—one that is necessarily short—will have a list of subjects that he or she feels are inadequately developed. To my mind, the Italian feminist movement is one such. Though Kogan is very good on such issues as the struggles for legal divorce and abortion, he gives little sense of the richness and sociopolitical importance of the women's movement during the last decade. I also think that more should have been included on the role and nature of the various Italian police forces. Certainly the struggle for the attainment of a police union—a subject which has vast implications for democracy and the left—should have been briefly discussed.

Despite these and other minor criticisms, I strongly recommend this unique and important book. It should be in every college library.

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JOHN R. LAMPE and MARVIN R. JACKSON. *Balkan Economic History, 1550–1950: From Imperial Borderlands to Developing Nations*. (Joint Committee on Eastern Europe Publication Series, number 10.) Bloomington: Indiana University Press. 1982. Pp. xviii, 728. \$37.50.

John R. Lampe's and Marvin R. Jackson's *Balkan Economic History, 1550–1950* seeks to test Alexander Gerschenkron's hypothesis that the less economically developed a country is, the more dependent it will be on institutional factors to encourage entrepreneurship and avail capital to new industry. The authors apply this test to the Balkans, which they define as those territories of the former Ottoman

and Habsburg empires that were reconstituted as Bulgaria, Greece, Romania, and Yugoslavia (data on Albania are sparse). The authors' claim that the period they cover is 1550–1950; in fact, three times as much space is given to the last hundred years as to the earlier three centuries.

The book is divided into three parts. In the first part, for which economic historian Lampe is mostly responsible, an ambivalent thesis on the role of European commerce in the neighboring Ottoman and Habsburg fringe territories is proposed, namely, that the Ottoman Balkans were part of an "open" economy (p. 13) but that the "presence of Western European interests" in both Ottoman and Habsburg portions of the Balkans was "simply too small" to make these territories anything but peripheries of the Ottoman and Habsburg core states (p. 16). Lampe then shows that the Black Sea region remained from the 1590s to the Treaty of Adrianople (1829) an economically partly closed territory: it was primarily a supply area of foods and raw materials for Istanbul (pp. 29, 83, 96). The dichotomy may result from the author's desire to disprove Fernand Braudel's thesis that the Mediterranean gives unity to the surrounding land area and that the varying density of Mediterranean and Central European influences divides the Balkans into two parts (p. 3). Lampe argues in favor of Balkan unity and defends the idea of the Danube as a unifying waterway (p. 5), a thesis of doubtful validity even after 1830.

In the second part of the book, devoted to modernization, Lampe probably correctly argues that the Habsburg borderlands and Balkan states achieved "intermediate position between the developed and underdeveloped economies by the start of the First World War." The rapid growth of new political structures and their underpinnings—educational endowments, transportation networks, financial institutions—was sufficient to allow an industrial mini-spurt between 1900 and World War I. But while these changes may have promoted an increase in real per capita income, they were inadequate to make growth self-sustaining. Although the Balkan states and Habsburg borderlands achieved a large degree of *modernization*, they failed to lay sufficient foundations for *development* (pp. 7, 11, 277, 322).

Directly or indirectly embracing Joseph Schumpeter's contention that banking and entrepreneurship provide the keys to rapid economic growth (p. 203), the authors devote a third of the second and third parts of their book to private and public finance. They neglect social and noneconomic variables such as tradition and opinion, models of development, ideology, and work ethic.

Lampe and economist Jackson are aware of some of the problems posed by the size and location of states. Lampe thus draws attention to the advan-

tages accruing to Romania from a larger overall population and from the concentration of urban population in a few centers (pp. 238–39). Jackson, who is largely responsible for the third part on war and economic development from 1912 to 1950, focuses on the problems of uneven regional distribution of land, labor, credit, and capital that resulted from different resource bases, from different historical experiences, from war and territorial annexation, and from political and ethnic rivalries. The issue of ethnicity was an acute problem in Yugoslavia but of secondary importance in Bulgaria. Bulgaria and Greece—the latter aided by an influx of entrepreneurial skills after Greece's war with Turkey and the population exchange agreements of the 1920s—were thus the countries with the most rapid industrial growth during the interwar era (pp. 402–33). The general industrial mini-spurts in the prewar and interwar periods were both "based on substituting domestic manufactures for imports within a limited national market. Both faced shortages of capital that would have held them back even without war and depression" (p. 587). But they were important, if not necessary, antecedents to the rapid Balkan economic growth achieved after 1950.

Published with support from the American Council of Learned Societies and with subventions and grants from the Foreign Area Fellowship Program, the International Research and Exchanges Board, the Council for International Exchange of Scholars, the National Science Foundation, the University of Maryland, and Arizona State University, this book is the best yet to appear in any language on the economic history of the Balkans since 1800. Even its virtues, however, may be flaws: for example, it thus includes 129 statistical tables, many of which occupy a full page or more, but they are almost never fully analyzed. Moreover, although the book is strongly empirical, it lacks a well-developed theoretical formulation. But if the book is a semifinished product—is the work of the historian ever finished?—it is clearly a work of considerable value.

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JAN F. TRISKA AND CHARLES GATI, editors. *Blue-Collar Workers in Eastern Europe*. Boston: George Allen and Unwin. 1981. Pp. xvi, 302. Cloth \$37.50, paper \$12.95.

The burden of this collective work edited by Jan F. Triska and Charles Gati is that the industrialization of Eastern Europe has brought into existence there a blue-collar working force that today constitutes the single largest social class and is increasingly at loggerheads with those who in Marxist doctrine consti-

tute its vanguard. This conflict has arisen despite an implicit social compact under which, in exchange for social peace, the party has promised full employment and improved living standards in a terror-free and egalitarian society.

A downturn in the world economy, together with growing scarcity of resources under socialism and the end of an early period of great upward mobility, have put the Communist rulers under renewed pressure to improve the efficiency of their centrally planned economics by means of market reforms. This would bring the elimination of over-employment, greatly increased wage and salary differentials, strict discipline on the shop floor, and, at least for a time, reduced living standards. The proletariat is understandably unenthusiastic. The regimes attempt to persuade it by putting greater emphasis on worker participation in the industrial decision-making process, but such participation will remain symbolic unless the party is willing to surrender the principle of democratic centralism. Nor will Soviet subsidization of East European living standards put off the day of decision. Strikes, riots, and other labor troubles, which have been rare during the thirty-seven years of Communist rule, are therefore expected by our authors to escalate significantly. In the end, Soviet control of Eastern Europe may be at risk.

Blue-Collar Workers in Eastern Europe is to be welcomed as a first effort at the systematic and comprehensive treatment of the industrial working class under socialism and, as well, for the substantial body of survey data it brings together.

I would have thought, however, that the implicit social compact could be as well or better understood as a reflection of Khrushchev's COMECON-wide policy of "goulash communism." Such legitimacy as, in the eyes of the workers, the regimes possess is probably based on the regimes' achievements in modernization as well as on egalitarianism and is in any case minimal for patriotic reasons. The apolitical character and the rarity of violent worker outbreaks (as well indeed as their spontaneous and explosive character) are to be explained primarily by the absence of autonomous worker institutions as well as by the heavy overburden of repressive agencies characteristic of Marxist-Leninist governments.

Neglected also is the impact of the international demonstration effect. For how else could a price increase affecting no more than 2 percent of the meat supply trigger a great Polish crisis after real wages had risen some 40 percent? Survey returns presented in the volume show that two-fifths of Polish manual workers thought their living conditions in 1970-78 had either not improved or had deteriorated and 67 percent evaluated the family budget as "highly strained" in any case. Nor could I

agree that worker dissatisfaction alone constitutes the chief threat to the stability of the East European socialist state. There were two major crises, the Czech in 1968 and the Croatian in 1971, in which the industrial workforce refused to follow the lead of dissident elites, and three others (Poland and Hungary in 1956, Poland in 1980), in which the unhappiness of the proletariat had to merge with the dissidence of the intelligentsia in order to produce regime destabilization.

So far as the reader can discern, no commentators were invited to the meeting at Stanford in May 1980, which put together the present volume. Instead, graduate students who were present during the proceedings offered written comments afterwards. No doubt this explains in some measure the lacunae adverted to above. Conference volumes are generally thought of as uneven in quality and fuzzy in focus. This one is no exception. The place of the anonymous referee in evaluating manuscripts submitted to a university press is in the conference volume normally taken by the tactful commentator. An earlier book edited by Triska and Paul M. Cocks, *Political Development in Eastern Europe* (1977), appeared without the papers of discussants who had been invited and even without reference to their having participated in that capacity. Both volumes, furthermore, include chapters not presented at the organizational conference without any indication of which were presented there and which not. It is clear that an editor must exercise the right of eliminating papers and commentaries that do not advance our understanding or our knowledge of the subject at issue and of adding others which do, but this certainly does not provide license to dispense with the service of competent critics in the production of conference volumes. In my judgment that would constitute a threat to the integrity as well as to the standards of the profession.

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MICHAEL HERZFELD. *Ours Once More: Folklore, Ideology, and the Making of Modern Greece*. (Dan Danciger Publication Series.) Austin: University of Texas Press. 1982. Pp. x, 197. \$17.50.

Kai pali dika mas. Every schoolchild in Greece knows by heart the folksong to which these words form the last line. That song, a lament on the taking of Constantinople by the Ottoman Turks, serves as a focus for a study with a dual but connected theme. Michael Herzfeld, an anthropologist, sets out to examine the development of folklore studies in Greece and to define its relation to the construction of a nationalist ideology in the new state during the nineteenth century.

The sociopolitical and cultural framework of his study will be familiar to those versed in the circumstances surrounding the development of modern Greece, whose territorial markers did not match up with the ethnic ones of a nation. Complicating matters further, the Greeks viewed themselves and were seen by others as directly related to two divergent cultural and socioeconomic realms: the civilization of the West with its spiritual roots in the classical Greek world and the Eastern imperial and theocratic domain of the Byzantine and Ottoman states. This dual perception generated a controversy among the culturally conscious segments of this nascent nation-state, which was reflected in their efforts to create a coherent and satisfying image of who the modern Greeks were. To those struggling to create the edifice of a new state the answer was clear: the nation (*ethnos*) was one of Hellenes. And this view, as the author rightly points out, was determined as much by foreign interests as by domestic considerations.

In using this commonly accepted duality as a framework, with its attendant ramifications in all areas of modern Greek society, the author finds another dimension to it. He posits a polarity between an outward-looking, Western-oriented "political Hellenism" and an introspective, "Romeic" outlook, with overtones of an Eastern way of life. The former is an intellectual exercise in nationalist ideology; the latter leans toward a psychosocial value system. To carry the duality forward in time in this form may be necessary in terms of the author's scholarly discipline, but it may also confuse the issue when it comes to understanding how a liberal, secular ideology became conservative and irredentist.

Folklore as a discipline in Greece did not become established until the second half of the nineteenth century with the work of Nikolaos Politis. By that time the country's national identity was bound up with the vision of the *megali idea*. Both the discipline and the ideology were affected by intellectual and political criticism emanating from Western sources. The failure of these now-liberated Greeks to conform to the political and cultural expectations of their European patrons resulted in the disillusionment of the latter with the Hellenes and their new state. Political affairs in the country came under sharp attack, and, most distressing of all, was the denial by some Westerners of a direct link between the modern Greeks and those of classical times.

In pained response the country's intellectual and political circles sought to demonstrate the unity and the continuity of their nation. Because of his interest in the development of folklore studies in Greece, the author in his discussion of the attack and the response begins with the latter, discussing the work of Zambelios, and then takes up the former, the

Fallmerayer controversy. In this way the importance of the historical moment (the 1830s) that the Fallmerayer episode represents is lessened. Greek intellectuals who reacted to the German's attack (including Zambelios, but more importantly Paparrigopoulos), first endeavored to establish ties between ancient and modern Hellas. By mid-century, shifting intellectual currents in Europe offered them an opening to establish another theme, continuity from classical to modern times with increasing importance accorded to the link between these eras, Byzantine civilization.

In a way this study might just as easily have been entitled "From the Politics of Revival to the Politics of Survivals." For the development of a modern Greek nationalist ideology runs from the didactic, impassioned efforts of Korais on behalf of the *ethnos* to Politis's defense of it through his research into the folkways of the *laos*.

This is a useful and worthy addition to the all too meager scholarly literature on the subject.

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CAROL IANCU. *Les juifs en Roumanie, 1866-1919; De l'exclusion à l'émancipation*. (Études Historiques, number 4.) Aix-en-Provence: Éditions de l'Université de Provence; distributed by Jeanne Laffitte, Marseille. 1978. Pp. 382.

Carol Iancu's book provides a very useful analysis of Romanian antisemitism before World War I, thereby helping to fill a significant gap in both Jewish and Romanian historiography. Against the backdrop of evolving independence and nationalism, the author presents a multifaceted account of Romanian attitudes toward, and treatment of, Jews. Iancu sees antisemitism as indigenous to Romania, with strong roots in the Orthodox church, but not endemic to the Romanian masses, who, he claims, frequently have come to the aid of their Jewish neighbors. He attempts to uncover the socioeconomic, political, and ideological, as well as religious and xenophobic, underpinnings of the official government policy of systematic discrimination against Jews. The main focus of the book is diplomatic and political developments, although the author ambitiously tries to employ psychological and intellectual history approaches as well with somewhat less success.

The most disappointing aspect of the book, to my mind at least, is the fact that it does not live up to its title and does not deal very fully with Romanian Jewry itself. Although there are chapters ostensibly dealing with the Jewish community and Jewish reactions to antisemitism, they merely whet one's appetite rather than satisfy it. The major Jewish actors on the scene are Adolphe Crémieux of

France, Moses Montefiore of England, and Benjamin Peixotto of the United States, rather than local Jewish figures. Iancu does not really substantiate his conclusion that Zionism, rather than emigration, was the most prevalent response to Romanian antisemitism. The author himself, however, acknowledges that an internal history of Romanian Jewry remains to be written.

This study, which emerged from a doctoral dissertation, is very thoroughly documented, especially with diplomatic sources, both in the text and in lengthy appendixes. It is largely based on French and Israeli archival materials; evidently the author did not have access to Romanian archives, although he mentions using the library of the Romanian Academy in Bucharest. A minor irritant in reading the book is to be found in lack of consistency in spelling and capitalization of certain proper names: for example, the name Julius Barasch/Juliu Baras appears in at least four different combinations, and a typographical error on page 149 has that same Jewish leader publishing a journal four years after his death.

All in all, however, this book is to be welcomed as a valuable contribution to the study of the rise of modern antisemitism in the late nineteenth and early twentieth centuries, because it enables us for the first time to place the Romanian case in a comparative framework with the parallel Russian phenomenon on the one hand and Western European developments on the other. I hope that the author, with his fine linguistic abilities, will continue to pursue his research on Romanian Jewry, perhaps even into the postemancipation era between the wars.

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EZRA MENDELSON. *Zionism in Poland: The Formative Years, 1915–1926*. New Haven: Yale University Press. 1981. Pp. xi, 373. \$35.00.

The establishment in 1948 of the Jewish state of Israel reflected the longing for, and belief in, an ancient homeland. It was a result of complex developments involving both Jewish as well as European history. Only the combination of external and internal causes could accomplish this fact, spurred on by the emergence of Zionism in the 1880s and 1890s. The term Zionism was first used at a meeting in Vienna on January 23, 1892, and Theodor Herzl made it synonymous with the political concept of Jewish statehood in his publication *Judenstaat*. The spread of modern antisemitism, which was often accompanied by bloody pogroms and culminated in the Nazi Holocaust, made Zionism popular with the Jewish masses. The Jewish homeland was to serve

not only as a place of physical refuge but also as a spiritual, cultural, and political base. Pronouncements about a Jewish homeland ultimately proved to be more convincing than either the socialist program of the left or the traditional-orthodox passivity of the conservative elements.

The intriguing rise, development, and final accomplishment of Zionism have been discussed in Walter Laqueur's *A History of Zionism* (1972) and David Vital's *The Origin of Zionism* (1975). Both authors provided Western readers with a general account of the movement, thereby preparing the ground for studies dealing with specific areas. Ezra Mendelsohn's *Zionism in Poland* should be seen as an extension of this literature, for it brings to light the lesser-known experiences of the Jewish people populating those areas of the German, Russian, and Austro-Hungarian states that after 1918–20 constituted the territory of interwar Poland. In 1921 in Poland the Jewish population was 2,853,318 or 10.5 percent of the country's population.

In that part of Europe that was to become Poland, Zionism emerged first of all in reaction to anti-Jewish excesses and policies, especially the notorious pogroms in Russia in 1881–82 and 1903. Its defensive purpose slowly developed into a political program aimed at the creation of a Jewish homeland, the preservation of Jewish identity, and the revival of Hebrew as the official language for Jews in diaspora.

Admittedly, the author's task was not an easy one considering the wealth of documentary material, four different political systems, and the near chaotic situation within the Jewish communities. He arranged the study chronologically for each political-administrative entity, ending his account with the year of Józef Pilsudski's coup d'état, the collapse of the fourth *aliyah* (wave of emigration to Palestine), and the decline of Zionist popularity among Polish Jews. Mendelsohn promised in the preface to extend the study up to 1939 in a second volume.

This study amounts to an in-depth and detailed description of the emergence and development of Zionism in Congress Poland, the Lithuanian-Belarusian territory, Galicia, and, after 1918, Poland. In each chapter the author provides the background situation as it existed in each state, examines the attitude of the local population toward Jews, and focuses on the prevailing mood among the Jews in general as well as within their political parties, organizations, and institutions. Such a broad scenario makes it easier to understand Zionist successes and failures. While comparing the situations of Jews under four different regimes, Mendelsohn asserts that they experienced very liberal treatment under the German military occupation of Congress Poland from 1915 to 1918 and a similar tolerant treatment in Galicia under the Habsburg rule. They encoun-

tered the most severe and restrictive conditions in Russia and to a lesser degree in postwar Poland.

Of special interest are the chapters discussing the relationship between Jews and other national minorities, especially the Ukrainians with whom close cooperation and support existed. This reviewer is impressed with the high degree of objectivity and informed knowledge the study exhibits.

The author elaborates not only on factionalism within the Zionist movement, but he also analyzes the reasons and causes that led to the splits and divisions among Jews. "In wartime Poland the emergence of Zionism as a factor in Jewish life was accompanied by the emergence of powerful anti-Zionist rivals, such as the Orthodox League and the Falkists, but in Lithuania and especially in Galicia such rivals were weaker" (p. 76). In Lithuania there was a strong Jewish socialist and Yiddishist tradition, but in Galicia neither the local Prilutski nor Zeire Zion could challenge the predominance of Zionism. The fate of Jews in Poland until 1926 steadily worsened. Despite its international and constitutional pronouncements, the government never cared much about the Jews, and Poles in general were either hostile or, more often, passive. The causes for such behavior were numerous: social, religious, economic, nationalistic, and demographic.

This difficult and complex situation together with prevalent external factors and never-ending frictions among the Jews themselves dimmed all attempts at unification. Furthermore, Polish Zionism never succeeded as a unified body but consisted of independent factions, and its history is that of a complex fragmentation. In the final analysis, Polish Zionism until 1939 was imbued with the messianic vision of a just, idealistic model society to be achieved in Palestine.

Mendelsohn's work represents a remarkable scholarly accomplishment in regard to detail and wealth of sources and literature used together with the handling of analytical synthesis. At the same time, the 373-page-long study crammed with minute details and deluged with terms, names, places, and notes makes for laborious reading rather than enjoyment, even for a seasoned and highly informed reader.

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G. A. FEDOROV-DAVYDOV. *Monety Moskovskoi Rusi (Moskva v bor'be za nezavisimoe i tsentralizovannoe gosudarstvo)* [Coins of Muscovite Rus (Moscow in the Struggle for an Independent and Centralized State)]. Moscow: Moscow University Press. 1981. Pp. 221. 1 r. 20 k.

G. A. Fedorov-Davydov of Moscow State University is well known as an archaeologist, historian, and numismatist whose interests range from medieval Central Asia to early Muscovy. In the present volume, Fedorov-Davydov pursues three interrelated objectives: (1) publication of the so-called Saransk hoard that was deposited around 1409 in a forest near what is now the village of Chernaia Promza, Bol'she-Bereznikov raion, in the Mordvin Autonomous Republic; (2) elucidation of the numismatic history of the silver coinage issued by Moscow and related centers in northeastern Rus during the late fourteenth and the first quarter of the fifteenth centuries; and (3) an analysis of the political significance of Rus coins of this era and especially how they reflect the changes in Moscow's relations with the Golden Horde and other Rus principalities. This book is *highly* recommended both for numismatists interested in the Juchid and Rus coinage of this period and for historians concerned with the evolution of Muscovy's political status during the reign of Grand Prince Vasilii Dmitrievich (1389–1425).

The Saransk hoard of silver coins, uncovered by chance in 1961, is composed of 109 Juchid coins (4.3 percent) and 2,438 Rus coins (95.7 percent). The latter component consists of coins issued by the Grand Principalities of Moscow (436 coins) and Nizhegorod (1,006), the principalities of Serpukhov (22), Dmitrov (41), Mozhaisk (4), Rostov (51), and Iaroslavl (3), as well as 658 unidentifiable coins, 215 effaced and worn coins, and 2 flans. Parts of the hoard are published in considerable detail in each of the first five chapters. For example, chapter 1 is devoted to the Juchid coins, chapter 2 is devoted to the Moscow coins, and so on. The book also contains a forty-four-page appendix with line drawings and photographs of all the main coin types discussed in the text. We are greatly indebted to Fedorov-Davydov for the publication of this large and important hoard. At the same time, the omission of the Nizhegorod, unidentifiable, and worn and effaced coins from this work is unfortunate. Partial publication of the hoard does not make the best sense numismatically or historically.

The attribution of Rus coins from this period is difficult and often controversial. Consequently, Fedorov-Davydov devotes much attention to the identification and approximate dating of these coins in the Saransk hoard. He has identified, for example, fifty-seven coin types among the Muscovite issues, which he has grouped into two chronological periods and an intermediary transitional phase. His attribution, typology, and chronology for most coins seems well founded; some interpretations, one suspects, are more speculative. In any event, Fedorov-Davydov deserves much credit for his first-rate

analysis of the coinage of northeastern Rus at this time.

Historians will probably be most interested in those parts of the book that examine the political message conveyed by the coins. The Muscovite coins of the 1390s, for example, usually bore the name of Khan Tokhtamysh on the reverse side, a visible symbol of Muscovy's dependence on the Golden Horde. During the first decade of the fifteenth century, however, Muscovite coins normally had no Juchid features on either side, a manifestation, according to Fedorov-Davydov, of Muscovy's effort to assert its independence from the Horde. The pictures on the Muscovite coins were also changed to fit new political conditions. In fact, Fedorov-Davydov points to eighteen different combinations of inscriptions and pictures from Muscovite coinage between the 1390s and ca. 1409 that reflect the evolution of Moscow's status vis-à-vis the Golden Horde. Fedorov-Davydov's analysis is highly sophisticated and, for the most part, convincing. He has clearly demonstrated that coins, when carefully examined by a master scholar, can tell us quite a bit about the major trends and minor nuances of political history.

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IU. G. ALEKSEEV. *Pskovskaia Sudnaia gramota i ee vremia: Razvitiie feodal'nykh otnoshenii na Rusi XIV-XV vv.* [The Pskov "Sudnaia gramota" and its Time: The Development of Feudal Relations in Russia, Fourteenth-Fifteenth Centuries]. Leningrad: Nauka. 1980. Pp. 241. 2 r.

Although Pskov was a provincial town on Russia's western border, its judicial charter (*Pskovskaia sudnaia gramota*) ranks as one of medieval Russia's most valued primary sources. The original charter was compiled in the late fourteenth century; Iu. G. Alekseev points out that it was the largest, most varied collection of Russian laws to appear in the three centuries since the pre-Mongol *Ruskaia Pravda*.

Readers looking for the actual text of the code (with modern Russian translation and article-by-article commentary) should consult volume 2 of the well-known "Pamiatniki russkogo prava" series. The late George Vernadsky provided an English translation in his *Medieval Russian Laws*. What Alekseev has done is to focus on those features of the compilation that reflect the "feudal" nature of the Pskov free republic. In many instances he compares individual articles with similar passages in the *Ruskaia Pravda* or other old juridical texts (*Zakon sudnyi liudem*,

Merilo pravednoe, various charters and statutes), emphasizing significant innovations in the Pskov document.

Alekseev describes the interrelationships of prince, communal mayor (*posadnik*), and hundred-man (*sotskii*), noting the considerable powers enjoyed by the latter representatives of the urban commune. He analyzes the code's provisions on judicial administration (criminal justice, commercial loans, suretyship, inheritance, land litigation) and examines three categories of individuals at the lower end of society in and around Pskov: the *smerd*, and the *naimit*, and the *izornik* (the status and characteristics of each have led to considerable scholarly controversy over the years). Although taking an impeccably Marxist-Leninist approach to all these topics, Alekseev does offer some interesting observations of his own. Some examples: the Pskov charter was the first to introduce the concept of criminal recidivism (in theft), and the *vol'naia rota* or "free oath" was literally that—one given voluntarily and not entailing serious consequences in case of refusal to take it. Alekseev describes a number of socioeconomic developments that led to the replacement of older, oral forms of evidence by written documents, but suggests that it took some time before the Russians understood that copies of these documents had to be made and kept in appropriate repositories if justice was to be best served. The final chapter (written as a twenty-four-page epilogue and not concerned directly with the *Pskovskaia sudnaia gramota*) presents a thesis that will dismay "vulgar Marxists": it proposes that two of Pskov's humble social categories—the rural peasant *smerdy* and the ordinary members of the urban commune, both lower-class segments of the population that should have been allies in the class struggle against wealthier citizens and upper-class landowners—were pitted against each other at the moment when Moscow threatened Pskov's independence! Moscow promptly took advantage of Pskov's internal discord, playing the various factions off against each other while usually posing as the defender of the downtrodden against those who opposed Moscow. Alekseev has not the slightest doubt that Pskov's eventual subjugation by Moscow was a progressive phenomenon, an event made necessary by the laws of historical development.

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N. A. KAZAKOVA. *Zapadnaia Evropa v russkoi pis'mennosti, XV-XVI vekov: Iz istorii mezhdunarodnykh kul'turnykh svyazei Rossii* [Western Europe in Russian Writing, Fifteenth to the Sixteenth Centuries: The

History of Russia's International Cultural Ties]. Leningrad: Nauka. 1980. Pp. 276. 2 r. 30 k.

One of the odd features about our perceptions of European-Muscovite cultural relations in the early modern period is that although Western perceptions of Muscovy have been carefully studied for many years, we know very little about Muscovite perceptions of the West. N. A. Kazakova, a respected student of Muscovite literary and intellectual history, has been working on little-known Muscovite writings about the West for over a decade. She has now produced a book that not only recapitulates her past work but also attempts to compile a list of all information about the West available to Muscovites in written form from the middle of the fifteenth to the end of the sixteenth centuries. All major types of evidence are considered: chronicles, translated and original literary texts, and diplomatic records. The author not only describes the information contained in these sources but also, by tracing the history of many texts and describing the manuscript miscellanies in which they are found, she gives the reader some idea of their audience and the literary context within which they arose.

Muscovite knowledge of Western Europe grew from virtually nothing at the beginning of the period to include the comprehensive geographical and historical information contained in translations of Marcin Bielski's "Chronicle of the Whole World," a few copies of which apparently existed in Muscovy after the 1560s. One of the earliest travelers' accounts, written by a visitor to the Council of Florence, vividly describes the mechanical marvels, the impressive architecture, and the realistic art seen along the way. More frequently information was confined to short chronicle entries about the arrival or departure of ambassadors, the Council of Florence, Ivan III's marriage to Sophia (Zoe) Paleologa, and so on. In the sixteenth century, this information was expanded by such diverse items as a list of European states arranged in an alleged order of precedence, an account of earthquakes in Italy, and a translation of Maximilian of Transylvania's authoritative account of Magellan's voyage around the world. The volume of reports by Muscovite diplomats and the questions the government asked of them, though formulaic, grew impressively, supplanting an earlier reliance on news told by foreign ambassadors. Nevertheless, one should not exaggerate Muscovite knowledge of Western affairs. All Protestant faiths, for example, continued to be referred to as "Lutheran," an indication also of Muscovite indifference to confessional politics. Further, apart from chronicles, the number of copies of works about the West (and presumably the readership) remained very small.

There are shortcomings in this useful book. Texts

on which Kazakova had worked previously are given a disproportionate emphasis. Although the author admirably refers to the work of Western scholars like Günther Stökl, who have written on the texts she describes, her knowledge of current European scholarship about Western events is limited. Finally, the book lacks a conclusion, which might have assessed the overall significance of her detailed findings. These reservations apart, Kazakova's book, furnished with two excellent indexes, is a most valuable contribution.

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ISABEL DE MADARIAGA. *Russia in the Age of Catherine the Great*. New Haven: Yale University Press. 1981. Pp. xii, 698. Cloth \$45.00, paper \$14.95.

By emphasizing "political history, including foreign policy and war, and administrative history, at the expense of economic history" (p. x) Isabel de Madariaga puts her book in the best tradition of pre-revolutionary biographies, particularly A. Brückner's *Katharina die Zweite* (1883) and Bil'basov's *Istoriia Ekateriny Vtoroi* (1900). Given this historiographical lineage, this review will comment on a few key points of interpretation where de Madariaga's opinions can be usefully compared to the well-known nineteenth-century analyses.

De Madariaga's account of the palace coup of June 28, 1762, accents the young Catherine's confident use of loyal troops to kill her husband and seize power. Catherine's success on June 28 was limited, according to the author, by the need to dissuade or prevent her chief minister, Nikita Panin, from imposing "some kind of constitutional limitation on absolute power, to be exercised by the aristocracy" (p. 39). What, however, were Panin's proposals? The "constitutional" nature of Panin's plans was not, seemingly, any program for a monarchy limited by the legislative powers of a royal council but a proposed series of administrative regulations designed to regularize the technical functioning of the machinery of government. Mention of the dangers posed by Panin argues for the premise that Catherine's grip on the levers of power was tenuous, an assumption integral to the overall arguments of Bil'basov and Brückner but continued by de Madariaga within quite the opposite context of a ruler willing to allow regicide, sure of her exercise of power, and clearly in command of the numerous factions at her court.

The empress's proposals for internal reform were advanced in the period of 1762–67 and discussed at the Great Commission of 1767. The policy initiatives discussed in 1767 and their partial implementation

during the "Reforming Decade" (1775–85) accent, the author argues, Catherine's genuine and sustained interest in administrative legality and continuity of policy. The author avoids the historiographical convention, fundamental to many nineteenth-century accounts, that requires Catherine be set in an idealistic pose for the 1762–72 period so as to require her "unmasking" in the post-1772 period. Yet even this major interpretive contribution is partially reneged by substantive elements of nineteenth-century historiographical premises. To explain why Catherine convened the Great Commission of 1767 the argument refers to the "precariousness of her tenure on the throne" (p. 161) and extrapolates evidence of the presumed threat from strong oppositional factions at court. Elements of the conventional Catherine, fearful of losing her throne, are occasionally mentioned side-by-side with the Catherine of de Madariaga's principal argument. The result is a historiographical image that is bifurcated or, at least, blurred.

Extensive attention is given to Catherine's diplomatic successes. Once again de Madariaga challenges the interpretation, current in much of the nineteenth-century literature on Catherine's foreign policy, that would see the nonaggressive policies of the early years (1762–68) change in the late 1760s toward plans for expansion into Turkish and Polish territories. The author argues that plans for expansion were at hand as early as 1763 (p. 189). De Madariaga's mastery of the evidence is nowhere clearer than in her superb account of the diplomatic origins and military prosecution of the two Turkish wars and the three partitions of Poland.

Potemkin is central to every analysis of Catherine in the 1770s and 1780s. Despite his importance as the premier statesman of Catherinian Russia, Potemkin's historical repute has suffered by reduction to eccentricities of personality or association with allegedly fraudulent schemes for colonizing southern Russian cities, the so-called "Potemkin villages." De Madariaga does much to "rehabilitate" Potemkin by taking him as the serious *homme d'état* that is warranted by his accomplishments in, among other fields, statecraft, military science, urban planning, and economic development. The straight lines drawn on the steppeland by Potemkin are splendid examples of the direct transfer of the precepts of enlightened rationality as applied to sophisticated plans for town planning, school systems, and numerous other projects for southern Russia. Potemkin's services re-emphasize Catherine's overall continuity of policy interests and fidelity to enlightened ideals of statecraft.

Relations between Catherine and her intellectuals are keyed, as de Madariaga suggests, to understanding the nature of their opposition to her. What were the bases for dissent in Catherinian Russia?

Consider the case of Nikolai Ivanovich Novikov (1744–1818). The prerevolutionary biographers, V. Bogoliubov and M. N. Longinov, argued that an enlightened, idealistic Novikov of the 1760s put misplaced hopes in Catherine only to be disabused of these illusions and obliged to find solace in a mystical version of Masonry. The Soviet biographer G. M. Makogonenko claimed that Novikov was a consistent enlightener, committed to the empress in the 1760s in expectation of reform, disenchanted in the early 1770s, and the originator of independent social action in the 1780s. De Madariaga tracks Novikov's career fully, but at the nodal points of interpretation chooses to endorse neither the pre-revolutionary nor postrevolutionary historiographical conventions; nor does she cite a variant or suggest a new line of reasoning. For example, the Novikov of the 1780s is both the enlightener devoted to the cause of popular enlightenment by means of his book business and the mystic given to the rituals and moral esoterica of Masonry (pp. 522–31). Without sufficient argumentative bases for Novikov's career, his rivalry with Catherine is not fully explicable. The nature of the intellectual opposition to the regime remains an unanswered question, but one that needs to be addressed for a full understanding of Catherine's Russia.

These comments suggest the nature of the challenges raised by the author's interpretation. In any comprehensive analysis there is plenty of room for debate among historians. Aside from particulars, the fundamental point is clear. Any serious consideration of the empress now begins with de Madariaga's study.

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P. S. SHKURINOV. *Positivizm v Rossii XIX veka* [Positivism in Russia in the Nineteenth Century]. Moscow: Moscow State University Press. 1980. Pp. 415. 2 r. 10 k.

Karl Marx took time to read Auguste Comte's voluminous *Course de philosophie positive*, but he chose not to undertake a closer examination of its basic premises and philosophical conclusions. He merely noted that Comte was the architect of a new idealistic orientation in philosophy and a new "conservative" ideology in politics. Lenin viewed various offshoots of positivism as part of a modern effort to revive philosophical idealism and fideism and to reconcile the modern revolution in physics with traditional religious thought. Guided by this tradition, Soviet philosophers and intellectual historians stuck to the strategy of condemning or dismissing

positivism as a current of decadent thought and misplaced emphasis.

The post-Stalinist "Thaw" produced a perceptible change in Soviet studies of positivism. It generated a strong interest in Comte's intellectual heirs, particularly in representatives of various neopositivist schools. It encouraged a closer re-examination of logical positivism and related orientations, which treated the logic of scientific inquiry and linguistic analysis as the central problems of modern philosophy. Favorable references to the contributions of Mach, Wittgenstein, Reichenbach, Russell, and Popper became common. The "Thaw" also encouraged serious inquiry into the history of positivist thought in Western Europe and in Russia. The new trend began in 1957, when M. P. Baskin and M. B. Kedrov published relatively favorable articles on Comte's philosophy of history and views on the unity of science.

P. S. Shkurinov's book examines the history of positivist thought in nineteenth-century Russia. It deals with the integration of positivist thought into Russian philosophy and with Russian criticism of the basic postulates of Comte's philosophical legacy. Several other recent studies have dealt with topics related to positivism, but this book is the most systematic and comprehensive. It divides the history of positivism in Russia into three periods. The first period, from the 1830s to the 1850s, was characterized by a slow and fragmented diffusion of Comte's basic ideas. Among the liberals, P. A. Ogarev presented the most typical view: he lauded Comte's devotion to science and "progressive" politics, but he objected to his unconcern with ontology as the heart of philosophy. During the second period, the 1860s to the 1870s, positivism became integrated into strong national traditions in nonacademic philosophy. Above all else, it played a major role in building a theoretical base for the social thought and cultural fermentation related to the great reforms of the 1860s. It existed, however, as a loose aggregate of separate currents—such as philosophical anthropology, natural-science materialism, and empirical philosophy—rather than as a strong and unified movement. At this time, Comte's ideas came to Russia through philosophical elaborations of J. Stuart Mill, historical interpretations of T. H. Buckle, and sociological discussions of H. Spencer and E. Littré. In the 1870s, the first Russians—led by K. D. Kavelin, G. N. Vyruhov, E. V. de Roberty, and V. V. Lesevich—tried to make positivism more than a depository of philosophical aphorisms. Shkurinov is particularly eager to show that N. G. Chernyshevskii and other "revolutionary democrats" were consistent critics of positivism, even though they favored the positivist belief in the intellectual supremacy of science. He claims that the populists, particularly in their elaboration of the idea of progress, were victims of an insufficiently

critical attitude toward positivism, even though they were not as "positivistic" as they appeared to some critics. The third phase, from the 1880s to the 1890s, was dominated by the rise of early neopositivist orientations. Here the author is particularly interested in the strong representations in Russian universities of "physiological idealism" and "physical idealism" as distinct varieties of neopositivism. He examines modern elaborations of positivist sociology, early influences of empiriocriticism, and the positivist bent of various theories of law.

Shkurinov's study adds a rich, challenging, and previously undervalued dimension to the historical study of Russian philosophy. Although it is broader in compass and deeper in analysis than standard Marxist studies of Russian thought, it is not without major flaws. The author does not present a full picture of the salient philosophical ingredients of positivism as a sociology and theory of knowledge. He seems to be preoccupied with two aspects of Comte's thought: the belief in science as the culminating point in the evolution of knowledge and human society and the identification of positivism with the "liberalism" and "progressivism" of "bourgeois politics." The narrow view of Comte's philosophical legacy has prevented the author from drawing a more complete, meaningful, and integrated picture of differences between neopositivism and neo-Kantianism and from avoiding ambiguities in identifying representatives of the positivist tradition in nineteenth-century Russia. The author's emphasis on the intellectual superiority of Marxism over positivism is a philosophical presupposition rather than an empirical generalization distilled from the hard facts of history.

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JOHN BARBER. *Soviet Historians in Crisis, 1928–1932*. (Studies in Soviet History and Society.) New York: Holmes and Meier. 1981. Pp. xiii, 194. \$34.50.

This study of Soviet historians during the first five-year plan offers a detailed analysis of the personalities and conflicts on the historical front during a time of crisis. John Barber does an excellent job of leading the reader through a maze of controversies and clashes between rival historians and theoretical approaches to the correct Marxist understanding of history. Although the substance of many of these debates may no longer be of interest to the historian, the process by which the party gained control over Soviet historians can be seen as a case study of the transformation of intellectual life in the early 1930s.

If this book were to have a subtitle it might well be "the evolution and the triumph of the principle of

partiinost." Barber clearly shows that the historical debates of the period were not based on disagreement over the fundamental relationship between history and politics: there was consensus that history was "the most political of all sciences," and that Soviet historians should support the policies of party and state through their writings; that is, that their historical works be imbued with *partiinost* or party spirit. The controversies centered on who had the right to determine the correct Marxist approach or party line in historical scholarship, and this question was settled only after the intervention of Stalin in the historical controversy in October 1931. It is within this context that Barber discusses the ironic fate of historian M. N. Pokrovskii, the dean of Soviet historians, who advocated the principle of *partiinost* but later (after his death) became a victim of this same Marxist-Leninist principle.

Stalin's letter to the editors of *Proletarskaia revoliutsiia* is seen as a definite turning point in the history of Soviet historical scholarship, and this is documented by the many historians who lost their positions and reputations or were forced to admit their errors. In the epilogue Barber indicates the ways in which new demands on historians brought about the orthodoxy of views characteristic of Stalinist historiography. More attention to this next five-year period might have been more in keeping with the title of the book, *Historians in Crisis*. The die may have been cast by 1932, but the crisis for Soviet historians continued with pressure from above to create a new patriotic and national history to coincide with Stalin's view of the past and the present.

Barber attempts to show that what was happening to Soviet historians was also characteristic for writers, philosophers, and cultural figures in general. This is a good point, but it could have been more forcefully made by spelling out the implications more clearly, rather than by simply listing examples. Soviet writers and Soviet musicians, for example, experienced a very similar crisis during the first decade of Stalinist rule and emerged with an orthodoxy of "socialist realism" that echoed very clearly the control over all cultural and intellectual activity.

The specialist in Soviet historiography will find this book to be a thorough and competent analysis and a thoughtful interpretation. A similar second volume developing the story of Soviet historians in crisis to the late 1930s would also be a welcome addition to our literature on Soviet historiography.

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NEAR EAST

ELTON L. DANIEL. *The Political and Social History of Khurasan under Abbasid Rule, 747-820*. Minneapolis:

Bibliotheca Islamica, for the Iran-America Foundation. 1979. Pp. 223. \$23.00.

The province of Khurasan occupies a unique place in the history of the early Islamic state. This vast area of deserts and oases, mountains and valleys, was on the extreme northeastern fringes of the Islamic world and yet twice within a century men from this area marched west to overthrow the established regime in the central Islamic lands, first during the Abbasid revolution of 750 and later when the Caliph Ma'mun defeated his brother in the civil war of 809-20. Between these two major irruptions, there was a whole series of revolts and civil wars in the area, involving many different sections of society, presenting a picture as complex and disturbed as that of Germany in the Thirty Years War. In his new book Elton L. Daniel has attempted to describe and explain these bewildering events.

The author's approach is traditional, and he gives us the accounts of the Muslim sources and notes their disagreements with a fullness reminiscent of the great Tabari himself. He also stresses that he has paid attention to the later Persian sources, often neglected, but these do not seem to have altered the picture significantly. Like Tabari, too, he is often content to give a variety of alternative accounts without adopting any of them or explaining how they come to be different. A fuller discussion of the attitudes and interests of the authors would have helped here. He is also on occasions unduly credulous, especially about the numbers given; when a source speaks of a peasant army of five hundred and seventy thousand (p. 142) we are entitled to suspect some exaggeration.

Daniel begins his narrative with a discussion of the Abbasid revolution and the role of Abu Muslim. The next two chapters deal with rural discontent and the relations between Khurasan and the central government in Baghdad, and the last section deals with the civil war at the beginning of Ma'mun's reign and the rise of the Tahirids. The account of the Abbasid revolt is useful, and the author stresses that it was a mass movement involving most sections of Khurasani opinion, not just the Arabs as M. A. Shaban has tended to imply. On the other hand, his contention that the success of the revolt was the result of thirty years careful planning by the Abbasids and their supporters will probably need revision in the light of A. Sharon's recent work. Explaining the later problems of the area, the author focuses on two factors, provincial resentment at government and, above all, taxation from Baghdad and social tensions within the province. In the first he is certainly correct, but the nature of the social discontents is much more difficult to define. It is clear that there were mass uprisings by many disadvantaged sections of the population, but why

they happened and when and where they did is not made clear. This is partly because the book lacks a sense of the geographical background. This is not entirely the author's fault; most of the area is now in northern Afghanistan or Soviet Turkestan and totally inaccessible to the Western scholar, and even good maps and photographs are almost unobtainable. Nonetheless, the social problems in mountain regions must have been different from those in oasis cities or pastoral steppeland. Similarly there must have been areas where conversion to Islam was rapid, those where it was much slower, and others where commercial interests must have affected social patterns significantly. None of these are fully discussed by Daniel. Despite this, the book is an interesting contribution to what must be a continuing debate.

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DONALD M. REID. *Lawyers and Politics in the Arab World, 1880–1960*. (Studies in Middle Eastern History, number 5.) Minneapolis: Bibliotheca Islamica. 1981. Pp. xix, 435. \$35.00.

Donald M. Reid of Georgia State University documents the rise and fall of lawyers in the politics of six Middle Eastern countries (Egypt, Syria, Lebanon, Jordan, Iraq, and pre-1949 Palestine), showing their significance and indicating that they have a story more interesting than one might expect.

Lawyers appeared in the Middle East only a hundred years ago, when European powers acquired a major role in local politics in the area. Indigenous legal systems had given virtually no scope to lawyers, but the European powers established new court systems that required them. These new tribunals included the Mixed Courts (which handled European interests), consular courts (sponsored by European diplomatic delegations), and national courts (which covered the local population except in regard to personal status and a few other matters).

Schools of law opened to answer the need for lawyers, beginning in 1868 with the School of Administration and Languages in Egypt. Lawyers then quickly acquired political importance: "The rise of the profession in the late nineteenth and early twentieth centuries coincided precisely with the heyday of Western commerce and conquest in the Middle East" (p. 395). Reid shows how the European presence fostered this development. Lawyers dominated the parliaments sponsored by European powers after World War I, they defended local interests against European encroachments, they enjoyed the confidence of the powerful landed class,

and they had vigorous writing and rhetorical skills; as a result, lawyers were able to dominate the political life of these Arab countries for a generation, from about 1930 on.

Their major role in the fight against colonialism led lawyers to expect substantial rewards in the period of independence. But they were disappointed, for the skills and connections that made them central to politics under European rule were nearly irrelevant later. Parliaments lost out to the armed forces and lawyers gave way to military officers; legal concerns lost out to economics and lawyers were pushed aside by engineers and economists. Reid chronicles the sharp decline of lawyers during the 1950s and foresees no likelihood of their return to power.

The author marshals a great mass of information in a clear and intelligent manner. But he writes in a plodding manner (for example, he ritually depends on "let us now turn to" or "we can now turn to" as a way of signaling a transition from one topic to another [pp. 13, 34, 94, and so forth]), and he overwhelms the reader with a barrage of small facts. Unless one is fascinated by the minutiae of bar association rivalries in Syria and career patterns in Jordan, four hundred pages of dense prose, charts, and tables will probably prove dauntingly long and boring.

Lawyers and Politics is the fifth volume in a series of "Studies in Middle Eastern History" published by Bibliotheca Islamica. This series also includes Reid's first book (a study of a turn-of-the-century Arab intellectual), and books on the first Persian revolution, the Akquyunlu, and seventeenth-century judicial administration in Egypt. Scholars of the Middle East should be grateful to the publishers of Bibliotheca Islamica for making studies available that otherwise, due to their extremely narrow scope, would probably go unpublished.

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AFRICA

IRIS BERGER. *Religion and Resistance: East African Kingdoms in the Precolonial Period*. Summary in French by M. D'HERTEFELT and D. CANNEEL. (Annales, series IN-80, Sciences Humaines, number 105.) Tervuren, Belgium: Musée Royal de l'Afrique Centrale; distributed by Humanities Press, Atlantic Highlands, N. J. 1981. Pp. xv, 181. \$14.00.

Iris Berger deepens knowledge of many societies of western Uganda, Rwanda, Burundi, and northwestern Tanzania by clarifying the relationships between religious change and other aspects of social and

political history over an extended time period. In the process she improves on past researchers by not isolating religious themes from broader societal concerns, investigating political and social developments from the standpoint of religious ideologies and institutions, thus providing a historical canvas incorporating the broader populations—not merely their rulers—once inhabiting this important African region. The societies Berger considers possess similar organizational forms and Bantu languages. One especially widely distributed institution is the *kubandwa* society, its members through spirit mediumship acting as valued intermediaries in time of personal crises between the human and spiritual worlds. In general the *kubandwa* societies, though possessing different deities, are similarly structured, with an easy entry at beginning levels, thus offering chances, especially for women, for rising to higher social and economic status. Berger examines the societies' development, rejecting as unsatisfactory previous beliefs of growth from pastoral conquests of agricultural populations, rather demonstrating their emergence from local, clan-based religious beliefs held essential to the cultures of the many small states existing during the early Iron Age. Between 1000 and 1500 the area underwent major demographic and economic change following entry of small groups of pastoralists leading great numbers of long-horned cattle. In resulting interactions, access to cattle replaced control of iron working as the prime source of authority. New ruling classes, allied with cattle keepers, gained control, overcoming strong opposition from existing political and social systems. Much resistance centered around religious symbols, but during succeeding years the competing systems compromised (with great variations among different regions), including old gods, and priestly classes, in the new cultural amalgam.

In elaborating this complex process Berger also contributes vital commentary to the long-existing controversy concerning possible historical content of the traditions relating to the *Abacwezi*, the mythical founders of many local states. Through fascinating analysis of the intellectual perceptions of early observers, particularly Harry Johnston, Berger demonstrates a change in the once existing consensus, the *Abacwezi* emerging as real human beings. During the twentieth century, African and other scholars worked through this racially based Hamitic hypothesis, turning the *Abacwezi* into a ruling dynasty of northern conquerors fundamentally molding regional history. Berger clearly proves lack of evidence for the entry of significant religious influences from intrusive groups, thus making a major contribution to the complex evolution of interlacustrine history.

Berger's scholarship is soundly grounded in broad archival work (Germany, Britain, Uganda,

Belgium, United States, Tanzania, Italy), wide reading in archaeological, anthropological, linguistic, and other related literature and oral investigation. Her arguments are advanced after cautious, rational examination of the most difficult evidence. The success of the effort naturally varies, perhaps succeeding the most for Rwanda, where evidence is extensive. Often, however, as Berger candidly admits (p. 80): "Unfortunately, it is difficult to discern the historical development of these themes." Nonetheless, Berger's effort should continue for some time to establish perimeters for existing research into interlacustrine African history.

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JIDLAPH G. KAMOCHE. *Imperial Trusteeship and Political Evolution in Kenya, 1923–1963: A Study of the Official Views and the Road to Decolonization*. Washington: University Press of America. 1981. Pp. xviii, 443. Cloth \$27.25, paper \$16.50.

Imperial rulers found few more satisfactorily malleable doctrines than trusteeship. It justified the seizure of African "Protectorates" and, when they became burdensome, their return. In Kenya, two questions arose: who should be the trustee and, what did trusteeship mean? The reply to the first was that, despite the British government's statements that the white community should share the imperial responsibility of protecting African interests, Britain considered itself the real trustee because the settlers had proved unreliable. The answer to the second was similar; His Majesty's Government decided what trusteeship meant at any given time. In 1923, it ensured the settlers continued political supremacy over the Indians, even though Indians outnumbered whites three to one, because in the well-known words of the "Devonshire Declaration" White Paper, "... the interests of the African natives ... must be paramount," that is, best served by white, not Indian rule. Jidlapth G. Kamoché shows how, in the following years, HMG used trusteeship to deny political advancement to settlers and Africans alike. It refused first to let the whites have the self-government they demanded so stridently and then, as African requests for participation in the various governing councils of the colony increased in the late 1920s and through the 1930s, trusteeship allowed Britain to tell them, with a paternal pat on the head, that they had not advanced enough yet to help govern Kenya. When the combined effects of the depression, World War II, and the "Mau Mau" rebellion eventually forced Britain to give Africans a larger role in the economic and political life of Kenya, and finally, much quicker than it had imagined, independence, it could de-

fend each retreating step as one more aspect of trusteeship.

Readers who know the standard works on Kenyan history, Bennett and the Oxford History of East Africa, for example, will be familiar with the themes Kamoche discusses in relation to trusteeship—the “Dual Policy” of separate economic, political, and social development for whites and blacks; land; taxation; African political representation; post-World War II multiracialism and the constitutional changes that led with gathering speed to independence—and will find his treatment of them objective and scholarly. For those who know Kenya less well and who are concerned about the political and administrative relationship between colonizer and colonized in multiracial dependencies, his book will be particularly interesting.

Its appeal to any reader, however, is reduced by lack of editing. The University Press of America performs a valuable service by publishing books that otherwise might not appear, but the absence of an editorial screen between author and reader makes some of its products too long and repetitive. Unfortunately, this is the case here. Proper editing would have increased the impact and utility of Kamoche's book.

JOHN SPENCER
Middlebury College

BASIL A. LE CORDEUR. *The Politics of Eastern Cape Separatism, 1820–1854*. New York: Oxford University Press. 1981. Pp. xiii, 314. \$39.00.

This readable and closely documented study brings together primary material from a wide range of sources, old and new, in the United Kingdom and South Africa; it is an expansion of an earlier period of the author's work, “Robert Godlonton as Architect of Frontier Opinion, 1850–1857,” published in the South African Archives Year Book in 1959.

This separatism is a much-studied subject, mostly by English-speaking historians: in the bibliography are ten theses either explicitly on separatism between 1820 and 1860 or on personalities involved in it. The author succeeds admirably in a work of synthesis, telling the story of a failure, at the same time keeping interest alive by competent handling of personalities. The endless and often tedious and confused debates over outright separation into two colonies or removal of the capital from Cape Town to the east are well set out: “The eastern separatist movement . . . was seldom truly eastern, rarely separatist and almost never a movement” (p. 281).

There were conflicting pressures on all of the actors involved: the permanent interests of settlers in expansion into Xhosa territory, and expenditure on public works likely to enhance development;

their fluctuating demands for government action when order on the frontier broke down; and the determination of the imperial government that *it* was not to be made to pay for the costs of settler expansion. What is interesting here, and not sufficiently developed by the author, is the development of Anglo-Afrikaner conflict, and the appearance of a sort of eastern Cape, largely Grahamstownian, jingoism.

The development of Anglo-Afrikaner mutual antipathy, though hardly surprising, was not a “natural” growth, and, as the author shows, there were anomalies: a “Midland axis” based on Afrikaner Graaff Reinet and English Port Elizabeth cut across the ethnic divide (p. 282). The author makes a reference to “the anger of the Boers over the government's failure to pass a stringent vagrancy ordinance in the mid 1840's” (p. 285). Does he mean that there was a characteristic Boer stance on such issues? For what we need to explain in much of South African history is the continuing hostility between settlers and Boers when their interests on a variety of issues were identical—access to more land, vagrancy ordinances, increased garrisons. A failed separatist movement could be used to tell us even more than does this excellent work. Separatism is a form of protonationalism, and its failure was only one example of the failure of English speakers to develop a political effectiveness comparable to that of Afrikaners.

JEFFREY E. BUTLER
Wesleyan University

ASIA AND THE EAST

WANG ZHONGSHU. *Han Civilization*. Translated by K. C. CHANG *et al.* (Early Chinese Civilizations series.) New Haven: Yale University Press. 1982. Pp. xx, 261. \$35.00.

Wang Zhongshu is one of the leading archaeologists in the People's Republic of China, and one of his main concerns for the past thirty years has been the archaeology of the Han period (approximately 200 B.C. to A.D. 200). Wang came to the United States in autumn 1979 under the auspices of the Committee on Scholarly Communication with the PRC (of the U.S. National Academy of Sciences) and the Chinese Scientific and Technological Association (of Beijing) to deliver a series of illustrated lectures, which have now been published in this volume. The volume exemplifies, first, the advantages of exchange programs of this kind and, second, the great gains in Han archaeology since liberation. *Han Civilization* will be essential for Chinese historians, and, because it focuses on China's first sustained

imperial period, it should be fascinating reading to all who are interested in the formative stages of civilizations.

The work consists of nine chapters: two on the Han capitals of Changan and Luoyang; one each on agriculture, lacquerware, bronzes, iron implements, and ceramics; and two on tombs. Under each of the topics, Wang summarizes the archaeological findings that pertain to the Han period; therein lies both the strength and weakness of the work.

For those who do not read Chinese and for those who do but who cannot keep up with the constantly growing archaeological literature or maintain a running synthesis of that literature, this volume is an excellent addition to one's library. Chinese archaeologists estimate that since 1949 "at least ten thousand Han dynasty tombs have been found" (p. 175). Not all of them have been opened, but those that have been excavated reveal the richness of a civilization that heretofore has been known largely through literary evidence. Americans are generally aware of the Han burial suits made of small jade plates sewn together with gold or silver thread and of the amazingly well-preserved woman in the tomb at Mawangdui. This volume not only gives other examples of comparable finds but also places these discoveries within the broader context of changing burial practices in the Han period. Before 1949 we had only a few pieces of Han lacquerware, but we now have dozens of examples. Wang, availing himself of these finds, traces the evolution of lacquerware technology through the 400 years of the Han. There is an added bonus in the lacquerware, because many of them bear inscriptions that allow the archaeologist to reconstruct the government-run institutions that produced much of this fine ware. Similar reconstructions are presented from the inscriptions on iron implements and bronzes. Thus, this volume contains much more than simple descriptions of the pieces exhumed from the archaeologically well-endowed China mainland.

The volume so impressively integrates the major Han findings of the past thirty years that one only reluctantly offers some adverse comments. The general reader would appreciate an introductory chapter that highlights the major developments of the Han period. The reader will discover, for example, that there is an implicit periodization that ties together the development of the various chapters, but these developmental stages should have been made explicit at the beginning of the work, if not by the author then by the editor.

There are interpretations in the text that derive from the ideological background of the author. To the extent that there is a social history theme that pervades the work as a whole, it is that of exploitation: the rich, the noble, and the powerful lavishly furnished their tombs only because of their ability to

deprive the poor of their grain, their cash, and their labor. The exploitation reflected in the tombs is simply a manifestation of the same practices throughout the society. No one denies that there was an elite in the Han period and no one denies that the elite lived a much better life than did the nonelite. Indeed, every complex society has such an elite, and every elite assures itself certain privileges. What Wang says in this regard is not new, but what is different in his presentation is the extent to which the Han elite allegedly relied upon "slave-laborers" (pp. 87–88) and "slaves" (p. 212). Wang offers no proof for the first example, and in neither case does he distinguish convict laborers from slaves.

There are other interpretative aspects of the text that warrant comment. For example, *yin-yang* and *wu-hsing* ideas are identified as Taoist (pp. 10 and 104); the author overlooks the fact that these ideas were integral parts of Han Confucianism. Again, although much of the archaeological data of the work comes from tombs and although there are two chapters on tombs, very little is said about the beliefs of the people who built the tombs. This aversion to dealing with beliefs is writ large in the clumsy description of the elegant bronze piece labeled: "the 'Flying Horse,' bronze galloping horse stepping on flying bird" (fig. 141). Winged horses are commonly found as the first pair of statues in the "Spirit Paths" leading to tombs. The winged horse and almost certainly the graceful and beautiful Gansu "Flying Horse" symbolize man's desire for physical ascent to heaven. The description of the piece in figure 141 will not lead the reader to that conclusion.

Unfortunately, the maps in figures 37 and 42 have been reversed. The headings are in the correct order to correspond with the references to them in the text, but the maps themselves do not match the headings. There are some erroneous transliterations in the text, but they will not bewilder the reader in the same way as the reversed maps.

The volume, in spite of these weaknesses, can be appreciated for the wealth of detail that it offers and for its fundamental importance to the history of early man. Each chapter consists of approximately ten pages of text followed by roughly sixteen pages of figures and maps (all in black and white); obviously, it is copiously illustrated. The reader has an excellent opportunity here to savor some of the features that have made the Han period one on which the Chinese have looked with great pride for the past two millennia.

JACK L. DULL
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HOYT CLEVELAND TILLMAN. *Utilitarian Confucianism; Ch'en Liang's Challenge to Chu Hsi.* (Harvard East Asian Monographs, number 101.) Cambridge,

Mass.: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge, Mass. 1982. Pp. xvi, 304. \$20.00.

The focal point of Hoyt Cleveland Tillman's study is the debate between the Neo-Confucian Ch'en Liang (1143–94) and Chu Hsi (1130–1200), the great synthesizer of orthodox Neo-Confucianism. Much of the importance and fascination of Tillman's study consists in the focus on the debate between these two men, who represent different approaches to the Confucian tradition. A side of the Confucian tradition that has not received major attention in Western scholarship is thus explored. The volume is primarily concerned with Ch'en Liang himself, but in order to develop the polarity of points of view interpretation is offered of Chu Hsi's perspective as well.

Tillman sees the major point of division between Chu Hsi and Ch'en Liang in terms of a difference in ethics. For Chu Hsi ethics was defined in terms of absolute and unchanging ends and was measured within the context of personal virtue and its cultivation. For Ch'en Liang ethics was a product of social context and thus defined in terms of the justification of the result. It is the latter orientation that produces the definition of utilitarianism.

The actual debate between Ch'en Liang and Chu Hsi was held through an exchange of letters as well as visits between 1182 and 1186. The debate is analyzed in terms of two primary issues. The first component, ethics in politics, is developed in terms of the major ethical issue between Chu Hsi and Ch'en Liang: absolute ends versus political ends. For Ch'en what was right and good (that is, possessing principle, *li*) was found in the context of a situation whose outcome was judged successful. For Chu Hsi right and wrong were not molded by situational utility. Instead, right and wrong were in the nature of things and were to be discerned through a process of personal moral and spiritual growth.

The second area of debate was in terms of Tao and the process of history. For Ch'en Liang, Tao was immanent in the activities of persons in various times and situations. The ramifications of this suggested an evolving and changing Tao through time. For Chu Hsi the primary feature of Tao was its durability, suggesting it was not subject to situational modifications. Chu Hsi viewed the age of the sages in antiquity as the point at which Tao was most fully manifest in history. Ch'en Liang, on the other hand, rather than upholding the Classics as the insights of sages, pointed to figures of later history as holding the key to understanding the growth and development of the Tao.

Tillman ties these two components of the debate together in terms of the issue of the retaking of northern China from Jürchen conquerors. Al-

though both men sought a policy that would permit the retaking of the north, differences in their policies can be seen in the light of their very different understanding of the Confucian teachings. Chu Hsi stressed self-cultivation and self-strengthening through learning before any action could be carried out. For Ch'en Liang it was not a question of moral rectitude first, but rather a policy of action that, were it to bring success in the retaking effort, would become the measure of its own moral correctness.

In addition to providing an excellent and important study of Ch'en Liang and the Chekiang orientation of Confucian thought during the Sung dynasty, the book makes an important contribution in the analysis of the changing historiographic consensus of Ch'en Liang, particularly the adjudication of his position in contemporary philosophical and historical literature. The only shortcoming, though an annoying one, is the extraordinary paucity of diacritical marks on Chinese terms.

RODNEY L. TAYLOR
University of Colorado,
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WINFRIED GLÜER. *Christliche Theologie in China: T. C. Chao, 1918–1956*. (Missions-wissenschaftliche Forschungen, number 13.) Gütersloh: Gütersloher Verlagshaus Gerd Mohn. 1979. Pp. 300.

The problem of reconciling Western ideology and Chinese reality is one that has engaged many Chinese intellectuals of the twentieth century. For Chao Tz'u-ch'en, the subject of Winfried Glüer's book, the particular shape of the problem was that of reconciling Christianity to China's tradition on the one hand and to its present condition on the other.

Chao, born in 1888 in Chekiang province, was baptized as a Christian in 1908. A mere two years later, he believed that all of China would be Christian within a generation and that China would find national regeneration through conversion. The transformation of Chao's attitudes toward Christianity and his perceptions of the functions it ought to fulfill in Chinese society might well serve as a map of twentieth-century Chinese thought. His early confidence in the potential of China for regeneration and transformation and his equating personal salvation and national salvation were very much in line with the concerns of other May Fourth era intellectuals. In the mid-1930s, he arrived at the conviction that the chief problems that beset China were economic and that the function of the church was to aid in the struggle for economic justice. And by 1956, Chao renounced Christianity as meaningless within the Chinese context.

The chief strength of Glüer's book is that it treats Chao's theology seriously on its own terms. Chao's

relations with Western religious movements such as the American "Social Gospel" and Western theologians such as Karl Barth are treated in some detail. The context into which Glüer places Chao is that of a Christian theologian who is grappling with questions of universal significance in a Chinese setting.

The question of the degree to which Christianity is a universal religion and separable from its Western cultural context is one that occupies both Chao and Glüer. Although Chao ultimately rejected Christianity, much of the work of his early years was devoted to a reconciliation of the two traditions. Chao regarded Christianity as a completion of Confucianism. In Chao's view, the Christian God spoke through the Chinese sages of antiquity—Confucius, Mencius, and Mo-tzu. Christ himself was the epitome of the Confucian ideal man, the *chun-tzu*.

Although Glüer's theological orientation is the source of much of the book's value, it is also the source of its chief weakness. Glüer is quick to point out European influences on Chao's thought but less ready to fit him into the context of contemporary Chinese intellectual activity. For example, Glüer traces Chao's conviction that the Bible is a historical document recording a changing religious tradition to the influence of German theologian Schleiermacher. Doubtless Schleiermacher is relevant. But perhaps as relevant would be the attitude of men like Chao's older contemporary K'ang Yu-wei that China's own sacred scriptures, the Confucian Classics, are themselves merely historical records. Had Glüer provided the reader a better sense of the community in which Chao functioned, we might have emerged with a clearer sense of who his audience was and of the Chinese intellectual discourse in which he was a participant.

Winfried Glüer has written a solid book illuminating a little-known area of modern Chinese intellectual history.

ANN WALTNER
Rider College

PAUL G. PICKOWICZ. *Marxist Literary Thought in China: The Influence of Ch'ü Ch'iu-pai*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xvii, 259. \$25.00.

Ch'ü Ch'iu-pai (1899–1935) was a commanding figure in the early history of Chinese communism, for several reasons. Posted as a newspaper reporter to Moscow (1920–1922), he was the first "May Fourth intellectual" to respond to the Russian Revolution at first hand in the years when Marxism-Leninism was winning the attention of young radicals in Peking and Shanghai. This experience propelled him, despite his youth, inevitably into the power structure of the fledgling CCP after his

return to China, trapping him thereby in the political-ideological entanglements of its disastrous Comintern phase. Ch'ü was instrumental in abetting the political downfall of his early patron, Ch'en Tu-hsiu, whose position of leadership within the party he inherited briefly, only to fall from grace himself, one of the dreary procession of scapegoats for the ongoing failures of made-in-Moscow policies.

But Ch'ü was also (first in point of time, foremost in respect to his inclination and contribution) a student and critic of literature as a social vocation—a revolutionary *wen-jen*. He played a leading role in the League of Left-Wing Writers in the early thirties, trying to maintain a middle position between its "realist" and "romantic" factions and to articulate a "proletarian" cultural outlook to combat the elitist ("bourgeois" and "Europeanized") doctrines of older May Fourth writers.

It is this aspect of Ch'ü's career that Paul Pickowicz focuses on in this sensible and sympathetic study. Ch'ü's fluency in Russian and familiarity with Russian conditions and the Soviet literary scene made it incumbent upon him to serve as a translator of Russian literature and Soviet revolutionary literary criticism, especially in those years when he was not preoccupied with politics. It was an obligation that he discharged with linguistic skill, but often, too, with a peculiar blend of studied ambiguity and stubborn silence that suggests intellectual reservations. Pickowicz must struggle throughout against his subject's reticences and lapses concerning such critical perplexities as the relationship between the old high culture and the hypothesized mass culture of the future; the question of whether writers, or intellectuals generally, are mere witnesses to history, or in some sense its makers; and the relationship between proletarian culture and the political party of the proletariat. Too often Pickowicz's well-informed discussions of the theoretical issues culminate with a resort to the lame formula, "Ch'ü said nothing about this (or that), but we may reasonably assume that. . . ."

Outside the confines of the Chinese communist cultural universe, Ch'ü's posthumous reputation rests in part on the apologia he wrote in the weeks before his execution by the Kuomintang in 1935—the testament of a "superfluous intellectual," as he had come to think of himself. In this reflective and sometimes candid document, Ch'ü affirmed his faith in Marxism but dismissed his own role as the negligible result of a "historical misunderstanding." Pickowicz, one senses, would like to refute that despairing final self-evaluation, but he presents little evidence to that end, and a good deal that indirectly supports it. Ch'ü's interest in Russia originated casually, in the fact that he could only afford to study at the tuition-free Peking Russian Language Institute; his youthful eminence as a Sovietologist

was clearly a case of the one-eyed king of the land of the blind; his renderings of Soviet literary history and criticism were mechanical rather than interpretive; he consistently failed (or refused) to relate Russian revolutionary experience to Chinese conditions and requirements.

All this Pickowicz concedes and documents. He counters, however, that Ch'ü's true importance as "the first to 'sinify' Marxist literary thought" derives from his "thorough knowledge of the problems of Chinese society" and his ability to "apply Western Marxist literary thought to uniquely Chinese conditions" (p. 191). This assertion might carry more weight had Pickowicz dealt more fully with Ch'ü's political opinions and analyses. But it is difficult to perceive in his critique of the "Europeanized" New Culture and its spokesmen or in his call for a "proletarian New Culture" based on the popular urban culture either an original or an especially insightful synthesis of Marxist theory and an understanding of China's social condition.

What does emerge unbidden is the portrait of an intellectual deeply imbued with the New Culture belief in the primary importance of cultural transformation, and with the hot nationalism of the May Fourth era. For Ch'ü, the Marxist critique of bourgeois culture became a convenient idiom in which to articulate his own anti-imperialist, essentially nativist and antimodernist opinions. Having accepted the critique in his youth, he spent the rest of his brief life trying to accommodate his values to Marxism's social categories and historical imperatives. In the end, Pickowicz's image of Ch'ü as a liberator of cultural energies is overshadowed by that of Ch'ü as the prisoner of an ideology that drained his creative vitality as fatally as tuberculosis undermined his physical strength.

JEROME B. GRIEDER
Brown University

STANLEY ROSEN. *Red Guard Factionalism and the Cultural Revolution in Guangzhou (Canton)*. (Westview Replica Editions.) Boulder, Colo.: Westview Press. 1982. Pp. xv, 320. \$32.50.

HARLAN W. JENCKS. *From Muskets to Missiles: Politics and Professionalism in the Chinese Army, 1945-1981*. (Westview Special Studies on China and East Asia.) Boulder, Colo.: Westview Press, with the cooperation of the Center for Chinese Studies, University of California, Berkeley. 1982. Pp. xxiv, 322. \$25.00.

These two books written by Harlan W. Jencks and Stanley Rosen present the latest studies of the Westview Press series on contemporary Chinese developments, which include some outstanding works of not only high academic quality but also considerable informational value. Both authors are

indeed to be commended for their painstaking efforts in examining the workings of two of the active participants (if not main protagonists) in the vicissitudinous drive for modernization and progress in the People's Republic of China since 1949.

Jencks's monograph deals with the economic, political, and social milieu in which the People's Liberation Army (PLA) has operated from its inception in the 1920s to today. Apart from appendixes and a select bibliography, the book provides a thorough critique on a motley group of subjects such as military professionalism, PLA modernization, Maoism, professionalism on trial, PLA structure, military industrial system, and PLA management. All these varying issues and topics, however, are admirably summed up in a concluding chapter (chap. 8) that sheds much light on the "professionalizing" trends in the PLA since the Civil War (1947-49) in spite of its traumatic political involvement in the Cultural Revolution (1966-70) as well as its manifest "return to barracks" (1969-74) and subordination to the control of the Communist party of China.

Rosen's treatise analyzes the factors that led to factionalism within the ranks of the Red Guards throughout the Cultural Revolution. In part 1, he discusses the mission and structure of Chinese schools and the relationships of Chinese students prior to the Cultural Revolution. In part 2, he depicts a vivid account of the rise of factionalism in Guangzhou (Canton) from both the individual and school perspectives in the midst of the Cultural Revolution. It is his contention that the ambitious goals of the Cultural Revolution could be achieved only through the mobilization of China's masses, that is, through the mobilization of school and university students. By the eve of the Cultural Revolution, however, a series of cleavages had divided China's school students, which resulted in factionalism among the Red Guards. By means of the Chinese official press, Red Guard newspapers, and his interview data on approximately one hundred former students from a variety of Guangzhou schools, the author gives new insight into the true nature of the Red Guards in particular and the Cultural Revolution in general.

If one may cavil at these two scholarly writings, both publications appear to be slightly pedantic. The former contains 10 tables, 9 figures, 137 abbreviations, 885 notes, and 7 appendixes. The latter consists of 28 tables, 1 figure, 35 abbreviations, 494 notes, and 2 maps, plus miscellaneous exhibits. By curtailing some of the appendixes, tables, and figures that are redundant, not relevant, or out-of-date, the authors would have more room for expounding their respective viewpoints.

J. CHESTER CHENG
San Francisco State University

THOMAS M. HUBER. *The Revolutionary Origins of Modern Japan*. Stanford: Stanford University Press. 1981. Pp. 260. \$19.50.

Thomas M. Huber makes two significant contributions to our understanding of the Meiji Restoration of 1868. First, he demonstrates that the dynamic figures in Choshu domain politics in the years before 1868 were social revolutionaries. The great motivating force of the men who controlled the leading domain in the Restoration was angry discontent with an unjust social system. Second, Huber establishes continuity between the ideas of the polemicists and terrorists of pre-Restoration Choshu and the modernizing reforms of the Meiji leaders in the central Japanese government of the 1870s. What once seemed abrupt and disjunctive forms a pattern in the author's hands.

The appeal of the book lies in that fact that, without oversimplifying, Huber discerns historical process out of the labyrinthine complexities of Bakumatsu and the Meiji Restoration. He reduces the attack on the Bakufu and the feudal system in Choshu to three well-defined, but interconnected, phases in which the confrontation constantly escalated. In the 1850s the struggle began through polemical writings on behalf of imperial reformism; in the early 1860s it continued through terror or limited violence; and in the middle 1860s it concluded with large-scale violence or civil war. An intellectual biography of the leader of each phase pinpoints motivation: the polemicist was Yoshida Shoin, the exponent of limited violence was Kusaka Genzui, and the leader of Choshu in civil war was Takasugi Shinsaku. Each died an early death, and it was Kido Takayoshi who implemented their egalitarian program in Meiji political councils of the 1870s.

These Choshu leaders were spokesmen for what Huber chooses to call the service intelligentsia. He believes that E. H. Norman's lower samurai is imprecise as a historical category. The disaffected activists were not all lower samurai, but they emerged from a narrow stratum of highly trained samurai of modest rank and faced intense competition for the few positions in domain government open to them. The Meiji Restoration was "a revolution carried out by this modern social class which found itself oppressed by the institutional configurations of the late feudal status quo." They were angered by the social injustice of a system that kept "all sectors resource poor for the sake of the lavishly endowed few." The dividing line in Choshu was samurai stipends of 200 *koku*. Those individuals above this level supported the conservative regime to the bitter end. The opposition came out of certain samurai houses below that level, as well as from schoolteachers who had "literacy with minimal social enfranchisement" and a few merchants whose hearts were in the public life from which they were

legally excluded. The individuals in control of the domain in 1868 after the Restoration limited samurai stipends to 100 *koku*. At the national level, through Kido, they helped establish a merit bureaucracy in the 1870s.

Huber's interpretation, therefore, is a fuller development of ideas suggested by Thomas C. Smith, who has written about the ideology of merit that led to disaffection in late feudal society, and of those ideas suggested by Ronald Dore, who has studied the tension produced by domain schools where merit counted and feudal society where it did not. The present book is most clearly a refutation of W. G. Beasley's contention that the Meiji Restoration was primarily a nationalist revolution. In Huber's view the Western challenge was incidental, an instrument to be used late in the game by an aggrieved social class.

For these particular leaders Huber makes his case, though it is possible that other men from other sections and classes, also involved in the Restoration, had alternate motivations. Another reservation is that the effort at comparative revolutionary theory does not quite come off. Parallels between the Choshu service intelligentsia and the Puritans, Jacobins, and Bolsheviks seem forced. Is it not possible that the Meiji Restoration was unique in history?

Huber's book will become a standard work on that landmark event. It discusses individual cases and takes us from the abstract level of general theory down to the motivations of particular men.

SIDNEY DEVERE BROWN
University of Oklahoma

MIKISO HANE. *Peasants, Rebels, and Outcasts: The Underside of Modern Japan*. New York: Pantheon. 1982. Pp. xiii, 297. \$9.95.

Modernization during the past century has made Japan prosperous, democratic, and well educated but by no means socially egalitarian. In a government survey listing ten major areas of daily life, citizens in 1978 expressed the least satisfaction with the degree of social mobility available to them. Mikiso Hane's *Peasants, Rebels, and Outcasts* illuminates the perceived lack of opportunity in twentieth-century Japan by highlighting the lives of people born in rural areas who benefited little from the national drive for wealth and power between 1868 and 1945.

The book draws together widely scattered information and summarizes scholars' recent findings about various nonprivileged groups that are rarely treated in surveys of the era. Lengthy eyewitness accounts by embittered or impoverished individuals enhance the impact of this volume as a spirited but nonideological narrative that offsets studies that concentrate on elites and their triumphs. Hane's thesis is that modernizing reform before 1945

"failed to improve significantly the condition of the people at the lower levels of the social order. In fact, for many it meant greater hardship and suffering" (p. 11). Their toil and trouble is abundantly evident on nearly every page of this relentless chronicle of misery.

Often relying on pungent anecdotes, the author gives a series of snapshots of disadvantaged farm women, village children, rural families, Burakumin outcastes, spinning-mill workers, prostitutes, and coal miners (many of the latter were convicts or Koreans). Because the method is mainly descriptive rather than analytical, not all readers will share Hane's confidence in generalizing from seemingly isolated examples. For instance, it is not clear how representative the accounts of farm life (pp. 34–36), boys' magazines (pp. 74–75), the Okaya Yamaichi strike of 1927 (pp. 196–200), or postwar schooling (pp. 259–62) are meant to be. Because the author favors impressionistic vignettes, often there is less sense of process or change between 1868 and 1945 than one might hope to find—particularly in the discussions of education, women, and Burakumin.

So broad is Hane's tapestry that he does not always tie up loose strands. Peasants are portrayed as rebellious on pages 21–27 but as "politically unsophisticated," "easily indoctrinated," and "bowing to authority" on page 56. Both images are accurate, but the enigma merits explanation. Many readers will be taken aback to learn that Japanese statesmen in the late nineteenth century sought "solely to adopt the *external* aspects of Western civilization" (p. 65; emphasis in original). And surely the difficulties of escaping outcaste status deserve closer scrutiny than is provided (pp. 147–148).

Peasants, Rebels, and Outcastes is particularly acute in its criticism of Japanese elites for their harsh treatment of underprivileged groups. Hane correctly notes that nineteenth-century liberal politicians, the supposed "champions of freedom and popular rights," condemned peasant uprisings and other commoner protests. The author brings out the failings of the prewar education system, the frequent exploitation of female labor in the textile factories, and the brutality of the wartime coal mines. Although he taps a great trove of muckraking literature in Japanese, Hane avoids lapsing into dogmatism. If there is an interpretive flaw, it may be that the author posits too sharp a break between prewar and postwar social conditions—a theme to be developed in his forthcoming study of the urban poor. Unfortunately, the cover illustration showing a turn-of-the-century spinning mill is reversed.

THOMAS R. H. HAVENS
Connecticut College

CHALMERS JOHNSON. *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925–1975*. Stanford:

Stanford University Press. 1982. Pp. xvi, 393. \$28.50.

Chalmers Johnson has produced a lively, detailed, and thoughtful review of Japan's industrial policy as it has evolved since 1925 at the hands of MITI (Ministry of International Trade and Industry) and its predecessors. Japan's rise from wartime devastation to the present economic heights cannot be explained, he argues, by any of the causes customarily offered: the simple operation of market forces, Japanese management techniques, special structural features, or unique cultural traits. It is due rather to the unequivocal acceptance of economic growth as the nation's top priority and the creation by superbly trained MITI bureaucrats of a rational industrial policy designed to achieve that objective, an industrial policy whose implementation skillfully balanced the advantages of planning with the efficiencies of the competitive market.

Specialists will be familiar with much of the information Johnson gives on the general setting of the economy, the nature of the bureaucracy and its role within the governmental system, the relations between business and the bureaucracy, the specifics of economic practices and policies, and the major disputes of MITI with other parties. New material and perceptions of particular interest are presented in two areas: Johnson provides the first account of MITI's inner history available to nonreaders of Japanese, and he sets forth in a most comprehensive fashion the thesis that all the individual practices and policies are interrelated parts of a system whose features primarily developed by trial and error out of the experiences of MITI bureaucrats, who, since 1925, have coped successively with depression, military expansion, wartime exigencies, and postwar reconstruction. Unfortunately, the very virtue of concentrating on the MITI bureaucrats may have inadvertently led Johnson to downplay the contributions of others. This seems a reasonable conjecture given that the two men who played the most important roles in the "high-speed growth" system—Ikeda Hayato and Ishibashi Tanzan—were not MITI products. Johnson also to some degree overestimates the number of innovations that arose in 1925–75. Many of the concerns, attitudes, practices, and policies that he believes originated as responses to the "situational imperatives" of the post-1925 period really are rooted more deeply in Japan's past.

This work rests firmly on an extensive exploration of the English and Japanese materials relevant to MITI. Johnson, however, seems more at home in postwar than prewar Japan. With regard to the latter period, factual errors creep in: for example, Sakonji Seizō was never "chief of naval staff" (p. 152); Kawai Ejirō did not "die in prison" (p. 97); agricultural landlords with large holdings were not "entitled to seats" in the House of Peers (p. 91);

Shijō Takafusa was not "created a baronet" but inherited the title baron (p. 95). There are occasions, too, where a translation is off, as in the Yoshino quotation on page 108, or where a Japanese passage does not support the point for which it is cited as evidence, as in the discussion about Vice-Minister Tokunaga's "inhibited . . . relations" with MITI Minister Ishii (p. 56). Such blemishes aside, Johnson's book contributes significantly to our understanding by illuminating hitherto obscure areas. It must be read by anyone seeking to comprehend the "Japanese Miracle."

ARTHUR E. TIEDEMANN
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TOSHIO NISHI. *Unconditional Democracy: Education and Politics in Occupied Japan, 1945–1952*. (Education and Society; Hoover Press Publication, number 244.) Stanford: Hoover Institution Press. 1982. Pp. xxxviii, 367. \$19.95.

In the eighty months from September 1945 to May 1952, Americans in military control of a conquered Japan attempted something unprecedented: sweeping and fundamental changes in the institutions and behavior of an alien people. Thirty years later, there is still no comprehensive, judicious history of this. Most books on the occupation read like an attorney's brief, much less interested in weighing all the evidence than in editing it selectively for or against.

Two exceptions are by Japanese authors: *Japan's American Interlude* (1960) by Kazuo Kawai, who experienced the occupation as a Tokyo newspaper editor, and *Unconditional Democracy* by Toshio Nishi, who was three years old when the occupation began, ten when it ended. Their books are similar in three respects: both find much to criticize en route to a generally favorable verdict, both show that the occupation's successes would have been transient without Japanese acceptance, and both are organized topically. In other ways, the two are quite different. Kawai has wider scope, but Nishi has greater depth of detail and background. Writing twenty years later, Nishi had access to vast quantities of recently declassified American archives and has used these with great skill (and careful documentation) to explain many points previously unclear or unknown.

Nishi's goal is something less than a complete history. As his subtitle implies, the focus is education broadly defined; he considers not only the school system but also the problem of teaching citizens the meaning of democracy and civil liberties. Within these broad limits his focus is wide but uneven: he writes nine pages (pp. 199–209) on aborted proposals to change the writing system but only two paragraphs on land reform (pp. 79, 287); he in-

cludes little on the new political system but much on food shortages, disarmament, rearmament, and antitrust policies as well as on textbooks, school boards, teacher unions, and radical students.

The point of the main title is the central paradox of the occupation: not simply that General Douglas MacArthur insisted on complete democracy but that he imposed democracy on Japan by dictatorial methods. Nishi criticizes these at length but concludes that "the contradiction was not a serious matter" (p. 297) and even that American embarrassment over it was "perhaps the most precious lesson of democracy that [they] left for the Japanese" (p. 139).

His sardonic interpretations of MacArthur's motives would often require change if he had taken into account the sense of urgency created by MacArthur's conviction that a long occupation would be self-defeating (mentioned only in an end-note on page 311). This view, rare among Americans, altered the whole course and character of the occupation. The book would also have benefited from considering the consequences of alternative courses. Could the same results have been achieved by less autocratic methods? Would Japan have fared better under a divided occupation like that of Korea or Germany? How did American policy in Japan compare with Japan's occupation policy in China or Java? What were the alternatives in 1950 (or later) to creating a Japanese self-defense force? Despite such omissions, Nishi has given us a scholarly, well-written, well-organized, and very useful book.

ROBERT M. SPAULDING
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BRUCE CUMINGS. *The Origins of the Korean War: Liberation and the Emergence of Separate Regimes, 1945–1947*. (Studies of the East Asian Institute, Columbia University.) Princeton: Princeton University Press. 1981. Pp. xxxi, 606. Cloth \$40.00, paper \$14.50.

With this massive work, Bruce Cumings launches a reinterpretation of the origins of the Korean War. If the richness of data and the level of analytical thoroughness evident in this first volume are maintained, we may expect to have, in time, the first full, systematic, and original account of postwar U.S.-Korean relations.

The author's familiarity with Korean language, culture, and history enables him to put the Korean War in the context of revolutionary nationalism in the country, not simply in the context of the global Cold War as virtually all other writers have done. He argues that Korean society was ripe for a social revolution following the end of Japan's forty-year colonial rule. There would have been political upheavals and social transformation even without the

occupation of the peninsula by Soviet and American troops. The revolutionaries—not necessarily all Communists, but termed “the Left” by Cumings—started establishing local and provincial committees as soon as the Japanese surrendered. They were intent on creating an independent mass state; the autumn uprisings by peasants throughout southern Korea in 1946 demonstrated the raw strength and organizational effectiveness of the mass revolution. Such developments predictably generated strong opposition, both in northern and southern Korea, and already by the end of 1946 the south had become the stronghold of the right, with its commitment to preserving “outmoded privilege” (p. 153). The result was that by early 1947 two distinct Koreas had emerged.

These developments, of course, took place at a time when the north was occupied by Soviet troops and the south by American, and thus the emerging framework of Cold War perceptions was inevitably applied to Korea. But for Cumings the importance of alien occupation lay in its relevance to the ongoing revolution; the Soviets on the whole supported it, whereas the American administration under General Hodge suppressed it. This was so, the book shows, because the opponents of revolution were more effective in persuading American occupation authorities to turn to them, as well as to the remaining Japanese colonial functionaries, for creating order and stability. Although the United States, the Soviet Union, China, and Britain had agreed on a four-power trusteeship (which was reconfirmed at the Moscow foreign ministers’ conference in December 1945), there was less and less inclination in Washington to implement it. The Korean right was able to affect occupation policy far more effectively than the “liberal internationalists” in Washington who remained committed to some framework of cooperation with Russia but who were put on the defensive as Cold War tensions grew.

The book makes a major contribution to our understanding of postwar U.S. policy. Its signal achievement lies in examining the origins of the Korean War as another chapter in the history of America’s response to third-world revolutions. The author, clearly reflecting the radical historiography of the 1960s, views the Korean War as a product of America’s support of counterrevolution, not a response to unprovoked aggression. Although the thesis itself is not entirely original—it can be found in non-American writings—Cumings provides substantial data to argue his case. One may disagree with Cumings’s argument; this reviewer, for one, finds his characterization of Korean politicians in terms of right and left a little ambiguous. Presumably, the former represented “nationalism, pure and simple,” and the latter “revolutionary nationalism,” according to a statement on page 88; but it is not

entirely clear where the author would put “conservatives,” “moderates,” “progressives,” or “tradition-minded reactionaries,” terms that abound in the book. Moreover, the evidence in the book suggests less a premeditated policy of counterrevolution on America’s part during the period under consideration than an absence of coherent policy, in sharp contrast to the meticulous preparation undertaken for the occupation of Japan.

Although readers will disagree with some of the book’s theses, no one will deny its value as one of the most comprehensive studies of “nation building.” The United States has pursued two principal goals since 1945: a global equilibrium among traditional powers on one hand, and the independence and economic development of new nations on the other. Korea was the first test of the “nation-building” policy. Cumings seems to be saying that the United States failed the test because its own liberal capitalist tradition had ill equipped the nation to cope with revolutionary mass politics and because its preoccupation with global power balances led its officials to view all local events in the context of the Cold War. In the perspective of the 1980s, one wonders whether the two Koreas have not been far more successful in emerging as “modern states” than most other third-world countries. But that is something the author may help us ponder further when his subsequent volumes are written.

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SUNG CHUL YANG. *Korea and Two Regimes: Kim Il Sung and Park Chung Hee*. Cambridge, Mass.: Schenkman. 1981. Pp. xi, 438, Cloth \$27.00, paper \$12.95.

The Korean peninsula exemplifies the axiom that the importance of a country need not be proportionate either to its size or power. Lying at the strategic crossroad of East Asia, Korea has historically served as the arena of conflict and competition among the major powers. Its partition in 1945 and the emergence of two rival regimes in 1948 have had profound repercussions not only on the Korean people but also on their more powerful neighbors as well. The fratricidal Korean War of 1950–53, in which a total of nineteen nations participated as belligerents, is but the most vivid reminder of the axiom. More important, the two Korean regimes, heavily armed and implacably hostile toward each other, may yet embroil their respective allies—Moscow, Beijing, and Washington—in a costly military adventure.

Given this background, it is not surprising that there is continuing scholarly interest in the various aspects of the Korean equation. The book under review is an attempt to examine recent Korean

history—from the late nineteenth century to 1979—through the lenses of macro and microanalysis. At the macro level, the author, a Korean-born political scientist teaching at the University of Kentucky Center at Fort Knox, scrutinizes historical and political backgrounds of the Japanese annexation of Korea in 1910, the circumstances surrounding the liberation and partition of Korea in the 1940s, the origins of the Korean War, and the sticky problem of Korean reunification. At the micro level, Yang delves into the backgrounds, careers, achievements, and failures of two political leaders—the late Park Chung Hee who ruled South Korea from 1961 to 1979 and Kim Il Sung who has monopolized power in North Korea from 1948 to the present. The merging of the two levels of analysis occurs when Yang attempts to explain the behavior patterns and policy outputs of the two leaders in terms of the larger setting in which they were enmeshed.

The author makes both methodological and substantive contributions to the growing literature on Korea. His is the first comparative study of Kim Il Sung and Park Chung Hee, and his attempt at an explicit linking of macro and microanalyses is praiseworthy. Yang's study can also be characterized as an exercise in psychohistorical interpretation of the two Korean leaders and the regimes they led. Some of his hypotheses are intriguing and insightful. The author displays an impressive command of the extant literature. Despite the lack of a bibliography, other scholars may benefit from a scrutiny of his extensive notes.

On the other hand, Yang's sources are predominantly secondary ones. The reader who is familiar with the literature is not likely to learn anything new about the backgrounds of either Kim Il Sung or Park Chung Hee. While the author's psychohistorical and psychopolitical inferences are certainly new, their plausibility is somewhat diluted by the uncertainty surrounding certain key facts. His efforts to generate new interpretations of the division of Korea and the Korean War and new ideas on how to bring about Korean reunification seem to have produced mixed results. While he does a good job of critiquing conventional wisdom, what he offers in its place is not entirely persuasive. Since space does not permit an elaboration of this point, suffice it simply to point out that the author's "third road" to reunification, for example, is no more realistic—in terms of its probability of being implemented—than the proposals that have been put forth by Kim Il Sung and Park Chung Hee (and Park's successor, Chun Doo Hwan).

Finally, the author's decision to devise his own system of romanizing Korean names and words—actually, a modification of the system devised by the South Korean Ministry of Education—was an unfortunate one. Not only is his system confusing to

scholars who are accustomed to the McCune-Reischauer system (which include *all* foreign scholars), but, more important, it does not do justice to the original Korean pronunciation.

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BURTON STEIN. *Peasant, State and Society in Medieval South India*. New York: Oxford University Press. 1980. Pp. xvi, 533. \$31.00.

Out of the apparent contradiction inherent in the concept of a "peasant state," Burton Stein constructs a complex historiographical analysis of South Indian society and politics during the Chola period. Concentrating on the "macro region" of Tamil civilization from the tenth through thirteenth centuries, he has written a work that should fundamentally change the conventional understanding of both South Indian history and peasant-state relations. Begun more than twenty years ago, this study lays out, analyzes, and rejects or modifies most established theories about South Indian society.

Stein's argument challenges conventional presuppositions about the nature of the state and society of South India. Focusing on the peasant locality, he asserts that the *nadu* was the "ethnic and ecological micro region . . . the basic unit of Chola society" (p. 105). This agrarian-based microregion was often self-governing and centered on Brahmin villages that provided social, political, and ideological unity. These *nadus* were the segments that were linked into a macroregion by a "ritually incorporative" king (p. 276). Thus drawing upon Aidan Southall's *Ahur Society* (1956), Stein presents the "pyramidal segmentary state" consisting of "differentiated elements in a single, universal moral system" (p. 22). Stein's reinterpretation of the relationship between peasant society and the state, which is a modification of an anthropological concept originally developed for African society, makes a convincing case for a new understanding of South Indian history.

Having established the inadequacies of conventional scholarship on South Indian society during the Chola period, Stein goes on more briefly to trace the social and political continuities and changes through the imperial Vijayanagara period (fourteenth through sixteenth centuries). He rejects the usual application of "feudal" to this period, seeing instead the continuation of the fundamental elements of the segmentary state. The most significant changes, however, were the decline of the unity of the locality and the development of new "supra-local" chieftains. Brahmin villages lost ritual and secular influence, and the unity of ethnic territoriality declined in the face of urbanization, the rise of the Hindu temple and sect, and the development of

translocal, left-right caste factions. The weak links between these new intermediaries and both the locality and the Vijayanagara ruler led, he suggests, to the eventual collapse of that segmentary empire.

Stein draws with admirable skill on social science—particularly anthropological—conceptions and the work of the younger generation of scholars writing on South India in order to reinterpret the published inscriptional evidence long used by South Indian historians. His approach of examining each element and conception in his argument afresh should be a model for all scholars to follow. His argument, often intricate and detailed, requires close attention and some prior knowledge of South Indian chronology. This reviewer would hope that Stein will now relate his conclusions about medieval South India to peasant societies in other regions of India and the rest of the world. This outstanding book should be a part of any research library that deals with either peasant society or South Asian history, society, or culture.

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A. APPADORAI. *The Domestic Roots of India's Foreign Policy, 1947–1972*. New York: Oxford University Press. 1981. Pp. viii, 244. \$14.95.

In India, the study of international relations owes much to A. Appadorai. His concern with the basic issues and ideas of politics surface in his many publications since the 1930s as well as in the journals he edited. He asks searching questions. The answers he essays are cautious and modestly expressed. So too in his latest volume exploring the roots of India's foreign policy in its domestic environment. The changing circumstances of the international arena receive scant attention; it is a welcome respite from some analyses that depict Indian foreign policy as purely reactive.

Appadorai specifies tradition and history, democracy, the demands of economic development, the pluralistic nature of society, and Jawaharlal Nehru's charismatic personality as the five essential ingredients of India's domestic environment. He devotes a chapter to each; not all are of equal strength. Having set forth the theory of democratic foreign policy making and the provisions in India for popular control of policy through Parliament and press, Appadorai discusses the practice. He finds the record poor. "Only in five instances that we are aware of until 1972," he says, "has the government been noticeably influenced by discussions in Parliament. Only in two did the government modify its policy in consequence." Some of the reasons for this disappointing performance in the early years no longer apply, such as public ignorance of international issues or wide consensus on Nehru's policy. A

study of the roles of Parliament and press in influencing Indian foreign policy in recent years would be a welcome one.

A succinct chapter on the demands of economic development traces several policy changes on export trade, foreign aid, and private foreign investment, over thirty years. Appadorai's assessment is balanced and adequately documented. He points out the vagueness of socialist thought in India and the pragmatism of policy. One important objective of foreign policy was to promote economic development at home. Neither pessimistic nor exuberant, he judges India's performance as "reasonably well done."

Americans as well as Indians are familiar with the complicating effects of an open, pluralistic society on foreign policy making. Appadorai focuses on two distinctive elements of India's pluralism: religious divisions among its peoples and the federal nature of the state. He asserts that "political parties in India have failed to perform one of their main functions, to bring together under their fold citizens belonging to different social groups. . . . The pluralistic character of Indian society partly explains this failure; the formation of communal parties flows directly from this failure." Who can disagree? He sees too a conflict between the concept of secularism—as compelling the state to be impartial to all religions and to deny religion as the sole basis of statehood—and the practice of communalism on the subcontinent. In this context he deals with Indian policy on Kashmir and the Israeli-Arab conflict, as well as varied Muslim opinion on the liberation of Bangladesh. Surprisingly, he does not take the opportunity for a general evaluation of India's policy on Pakistan in which domestic roots and foreign policy are so closely intertwined. Instead, we get a meaty and well-documented chapter on the effects of federalism. Appadorai shows how the special interests of a few states have inhibited the ability of the government of India to make agreements and contract obligations with neighboring countries, such as Sri Lanka and Bangladesh. His conclusion is unexceptionable—"the implementation of foreign affairs requires the willing cooperation of the units."

The concluding chapter on Nehru could well be omitted. Appadorai leaves the impression that Nehru's charisma was somehow separable from his country. He could have explained Nehru's leadership better as a superb ability to understand tradition and history while articulating India's needs before a contemporary international audience.

Readers should not expect illumination on India's foreign policy in the post-Nehru period from Appadorai. Though he assumes that the domestic roots of foreign policy remain fundamentally unchanged, he does not explain how they influence Indira Gandhi's policy, nor why he chose 1972 as a cut-off date. His conviction that a good foreign

policy must rest on sound domestic policy, and his doubts about the latter in the 1970s, may explain his silence. Perhaps his next volume, expected soon, will rectify the omission and explore another question raised only passingly in this—"does India have the ability to use foreign policy as a tool to achieve her national interests?" We hope too that the next volume will be tightly knit. The main disappointment in this one is the absence of linkage between Appadorai's introductory theorizing on foreign policy, national interest, state resources, and the like, and subsequent chapters. *Domestic Roots of India's Foreign Policy* remains a collection of separate lectures, some of which are most interesting.

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K. M. DE SILVA. *A History of Sri Lanka*. Berkeley and Los Angeles: University of California Press or C. Hurst, London. 1981. Pp. xx. 603. \$38.50.

This will long remain the definitive one-volume history of Sri Lanka. It is written in crisp English with never a wrong word or usage. The reader is kept abreast of the economic evolution of the society and is always made aware of the relationships of art, architecture, and literature to the island's history. The legends regarding Sri Lanka's origin are skillfully projected upon the growing body of archaeological findings. The hydraulic civilization of the Anuradhapura and Polonnaruwa eras, the cultural continuum of Buddhism and the emerging national myth embodied in the *Mahavamsa* are thoroughly covered. Some fresh ideas are advanced regarding the retreat south after 1200. K. M. de Silva sorts out the confused period of rival kingdoms and the Portuguese intervention. His comprehensive evaluation of the Portuguese and Dutch impact on Sri Lanka documents their important legacies.

Serious British intervention in Ceylon, he notes, reached its climax with the fall of the Netherlands to French Revolutionary forces. Sir James Stephen's evangelical zeal forced British officials against their best judgment to terminate the policy of protection of Buddhism inaugurated in the Kandyan Convention of 1815. The Colebrook-Cameron reforms opened an era of progressive administration, and Colebrook's approach to the Kandyans is seen as comparable to Lord Durham's attitude towards the French Canadians.

By the end of the nineteenth century, de Silva feels, Britain had produced a stable colony, but while plantations flourished, they swallowed up village reserve lands and inundated the hills with Indian Tamil laborers.

He explains why Ceylonese religious reform and political nationalism showed little mutual reinforcement in the nineteenth and early twentieth centu-

ries. A momentary confluence during the 1915 riots fell apart immediately. When the new Ceylon National Congress was formed, the author points out, it soon was dominated by a "constitutionalist" elite. The ethnic unity of the riots era gave way under the "divide and rule" manipulations of Governor Sir William Manning, causing tensions between Sinhalese and Ceylon Tamils.

Under the Donoughmore Constitution of the 1930s, the old guard continued to dominate, but the shape of things to come became manifest when Marxists were elected to the State Council and when religious and nationalist demands were put forward by S. W. R. D. Bandaranaike's Sinhala Maha Sabha. A strong personality, D. S. Senanayake, displaced the aging D. B. Jayatilaka as leader of that Council and ably directed the policies leading to independence in 1948.

S. W. R. D.'s era is seen as a time of great political changes shaping the prime minister rather than being guided by him. His assassination, however, promoted the myth of great leadership. There is no mention of his alleged loss of nerve during the 1958 communal troubles.

Sirimavo Bandaranaike, succeeding her late husband as leader of the Sri Lanka Freedom party, turned toward a more socialist, more communal, and more internationally left-leaning policy. This, the author feels, produced an oppressive regime leading to an insurrection of youth in 1971, to alienation of the Tamil minority, and eventually to the massive victory for the more conservative United National party in 1977.

De Silva's encyclopedic knowledge of Sri Lanka's history is brought to the reader with objectivity. His account gives recognition to the important and ancient role of Tamil people and culture. Examining British rule, he eschews the easy line of anticolonialism, documenting not only the British failure to understand the Sinhalese mind but also the British *pax*, its order and justice.

The treatment of the Marxist movement is fair, noting their parliamentary leadership as well as their opportunism, sometimes desperate, in a traditionalist society. There are valuable maps and appendixes.

CHARLES S. BLACKTON
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W. H. OLIVER and B. R. WILLIAMS, editors. *The Oxford History of New Zealand*. New York: Clarendon Press of Oxford University Press. Pp. xiii, 572. \$39.95.

Fifteen historians and one political scientist have worked together successfully to produce this excellent book. Each has drawn on the scholarship in the arts, letters, and social sciences of the past ten to fifteen years and has distilled his or her findings

into the mold of social history. In amplification W. H. Oliver writes, "The analysis of social systems, of the relationship of classes and groups, of stratification, mobility, consensus, and conflict" are to be found in the book, even in chapters on economics, Maori affairs, politics, and intellectual and cultural affairs. Missing are biographical sketches of such famous New Zealanders as Julius Vogel, "Big Dick" Seddon, Michael Savage, and Peter Fraser. The editors have planned a comprehensive history, and, although the chapters are uniform in size, there are a variety of interpretations by the contributors. Each also provides a plethora of footnotes, a generous bibliography, and excellent charts and maps.

Janet M. Davidson has written with great skill on the Polynesian foundation. She proves that the Polynesians reached the west Pacific as long as 4,000 years ago, and they settled in New Zealand around A.D. 800. She proves that the average length of life of a Maori was twenty-eight years. Their religion left little material trace, and archaeology provides little data on ritual. She writes that only one aspect of the death ritual—the shedding of human blood in mourning—was observed by Captain Cook in 1770.

Jeanine Graham provides an excellent chapter on "Settler Society." She discusses settler mobility, the life of shepherds (who were especially welcome if they brought their sheep dogs), class distinction, colonial fashions, food, housing styles, religion, and education. She also comments on musical and theatrical interests, egalitarianism among the workers, and the observation that life was physically and emotionally demanding for the pioneer women. The society was definitely rural and grew to about half a million by the end of the century. How this group, dependent on Great Britain for immigrants, exports, and political institutions, grew to an urban society of over three million today is explained by the next eleven chapters.

Erik Olssen's chapter "Towards a New Society" picks up from Jeanine Graham's chapter and carries the social analysis from 1900 to around 1936. He shows how modernization is tightly linked to the concept of industrialization. Age and sex structures had lost their frontier or preindustrial characteristics by 1930, even though half of the population lived in towns of under 8,000 people. The newly shrinking urban family "modified its traditional patriarchal form" (p. 258).

Michael King continues the history of the Maori in his chapter "Between Two Worlds" (1900 to 1936). In 1900 the Maori population of some 45,000 was isolated, thereby reinforcing their local and tribal identity. By the 1960s they had become a predominantly urban people. Maoris lost land by confiscation, by unfair purchase, and by "improper pressures exerted through the Native Land Court" (p. 284). Prowess in battle was highly prized in

Maori tribes, and Maori units in World War I and World War II fought bravely and with unusual distinction.

The coverage of twentieth-century New Zealand is particularly good, especially the economic and social developments from 1945 to 1980. After 1945, forestry products joined the three traditional export categories—wool, meat, and dairy produce. In the final chapter, "The Awakening Imagination," W. H. Oliver concentrates on cultural developments. After World War II a literary magazine, an orchestra, and a small government fund for writers came into existence, which led to an upsurge in the arts. The role of the New Zealand Broadcasting Company in this upsurge is properly appreciated. The developments in higher education are strangely missing, and, although Keith Sinclair is mentioned as poet and historian, his efforts in founding and serving as editor of the *New Zealand Journal of History* are not mentioned, and he is not one of the authors of a chapter of this book.

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RODERICK PHILLIPS. *Divorce in New Zealand: A Social History*. New York: Oxford University Press. 1981. Pp. 154. \$10.95.

This book by Roderick Phillips reflects the strength and weakness of its genre. It is a survey combining a historical outline of the evolution of divorce legislation, a statistical analysis of divorce petitions, and a case study of nineteenth-century Auckland. The first two chapters, which are a succinct account of the social pressures and attitudes surrounding divorce legislation, raise a multitude of crucial issues. These cannot all be probed to the depth acceptable to a reader who is fascinated by the topic, and therein lies the potential fruitfulness of these chapters. For example, given their different legal tradition, were the Scots decisive in replacing the English-based 1867 act with that of 1898, which was decades ahead of contemporary English legislation?

The relationship between women's suffrage and more liberal divorce is discussed, and Phillips notes that some conservative politicians, concerned for the moral values of family life, opposed divorce but supported suffrage as women would vote to protect such values. Women appalled at American moral turpitude often campaigned for equal access to existing grounds for divorce rather than for an extension of grounds. The extension to include incarceration for insanity produced a plethora of prejudice. A doctor recommended that, as neurotic people attract neurotics, neither party to such a divorce should be permitted to remarry. In the

thirties, despite strong pressure the judiciary failed to stop the misuse, through connivance of the parties involved, of the noncompliance clause that made a farce of the generally restrictive legislation. Divorce was gradually changed from being a moral punishment for matrimonial faults to a device that could legally end a broken marriage.

Phillips believes that a "religious" concept of marriage with preindustrial economic imperatives gave way to a utilitarian view emphasizing personal satisfaction. He sees colonial marriage in New Zealand as "pre-industrial," noting the dual role of employment-marriage agencies. This view needs stronger support. His statistical analysis on the duration of dissolved marriages, the size of the families involved, the religion of the parties, their age at marriage, social status, and the level of remarriage will make useful comparisons, a task Phillips explicitly eschewed. If general demographic profiles had been available, some of the tables would have been more meaningful. Due to the unevenness and nature of the evidence, the Auckland case study does not contribute significantly to the argument, but the letters of estranged wives recreate some of the more personal aspects of divorce.

In places Phillips's evidence is thin, but it is difficult to ascertain whether this is due to the limited purpose of the exercise, to its absence, or to its unavailability given the time at the disposal of the author. The value of the book, however, far outweighs its deficiencies. It provides an excellent introduction to the problem of divorce and raises questions that should stimulate further research. Phillips's work in European history has enabled him to place the myriad of issues into historical perspective.

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UNITED STATES

PETER DOBKIN HALL. *The Organization of American Culture, 1700–1900: Private Institutions, Elites, and the Origins of American Nationality*. (New York University Series in Education and Socialization in American History.) New York: New York University Press. 1982. Pp. viii, 325. \$35.00.

This ambitious and provocative book illuminates the role of Northeastern elites in creating a national culture. Focusing on the private corporation, Peter Dobkin Hall examines its growth in education, religion, law, and medicine as well as economic life. His achievement is uneven but impressive. Rejecting historiographical folklore about a "decaying Brahmin caste," he documents the disproportionate and

persistent influence of New England institutions in American life. Avoiding the current tendency to ignore elites and exaggerate working-class autonomy, he locates the ubiquitous "search for order" in a specific class and region. While the book contains serious flaws, it represents an important step toward what Robert Berkhofer has called an "organizational synthesis" of American history.

In Hall's view, elites began to turn toward corporate organization as a result of the "social crisis" affecting older Northeastern communities during the late eighteenth century. Population pressure on diminishing land, intensified by partible inheritance laws, strained family and community ties. Prosperity and social consensus declined. The Federalist Standing Order, shaken by popular unrest, faced dissolution amid the disorderly, democratic marketplace of nineteenth-century social relations.

What Federalists lost politically they sought to regain economically and culturally, by turning to voluntaristic forms of corporate action: business partnerships and joint ventures, private colleges, professional organizations, and benevolent societies. Given new legal protection and administrative flexibility, colleges in particular flourished. Harvard, Yale, and a handful of other private schools won unprecedented national influence. "The development of a national culture became the peculiar task of a small set of privately funded institutions in states in which public interference in the affairs of private corporations was minimal" (p. 122)—in Massachusetts and Connecticut, rather than Virginia or New York. Led by reformers like Timothy Dwight and Josiah Quincy, Yale and Harvard helped to nationalize "character" as a key to cultural leadership. The professionalization of medicine and the organization of evangelical benevolence played similar roles in cementing the authority of established elites. The "age of the common man" witnessed the transformation of the Standing Order from a scattering of official elites to a national economic class.

Following the seminal work of George Fredrickson, Hall argues that the Civil War unified this "national elite," reintegrating its authority along corporate lines. Evangelical and metropolitan strains joined in the "culture of organization" reinforced by the war. Reaffirming elite authority, the war also strengthened its material base in the hands of Northeastern investment bankers. After the war, Charles William Eliot and other educators championed the modern university as the home of a trained national elite. And by the early twentieth century, "Progressive" reformers had consolidated the "moral revolution" begun by the Federalists: the restoration of elite cultural authority on a bureaucratic basis.

Despite the many strengths of this book, there are some serious problems. Written in a leaden style, it

is also marred by numerous typographical or spelling errors; most are merely annoying but some subvert meaning. Hall overlooks much historiography that questions the importance of Darwinism in business apologetics; he also treats "Progressivism" as a monolith. And he exaggerates the national influence of Northeastern institutions. This is especially obvious in his tendency to treat the evangelical ethos as if it were a New England creation. "Character" was a unifying thread in nineteenth-century socialization, but it was spun by Kentucky Methodists and Alabama Baptists as well as by Connecticut Congregationalists.

The most serious problem involves the larger historical significance of the process Hall describes. He embraces without evidence the familiar fantasy that elite recruitment has been "democratic" and "meritocratic"; he also implies that "the masses" were smoothly integrated into the emerging bureaucratic system. In this he shares the myopia of most functionalists. His Eliot is the broad-minded apostle of general education, not the man who in 1877 drilled Harvard riflemen to defend Brahmin property against "rioting" strikers. The point is not to resurrect the stale debate over "consensus" and "conflict" but to restore some complexity to the historical record. The triumph of bureaucratic authority was never a matter of smooth integration but of social, cultural, and psychic strain—among the victors as well as the vanquished.

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DORA P. CROUCH *et al.* *Spanish City Planning in North America*. Cambridge: MIT Press. 1982. Pp. xxii, 298. \$35.00.

JOHN R. STILGOE. *Common Landscape of America, 1580–1845*. New Haven: Yale University Press. 1982. Pp. xi, 429. \$29.95.

Organized like an anthology, *Spanish City Planning in North America* surveys Spanish influences upon Spanish colonial town founding. Through a new translation of the "City Planning Ordinances of the Laws of the Indies," issued in Spain by King Philip II in 1573, Axel Mundigo presents sections of the laws that "have never been translated before into English" (p. 3). Further, he corrects some errors in previous translations published by Zelia Nutall in 1921 and 1922. A following commentary presents a historical context for these laws.

This is followed by essays on three American cities—Santa Fe, New Mexico, St. Louis, Missouri, and Los Angeles, California—by Dora Crouch that rely heavily on secondary sources. Based on previ-

ous publications, the survey of Santa Fe presents virtually no new information. It is unfortunate, for instance, that such sources as the Archive of New Mexico, which contain numerous Spanish documents, were not studied for new material. The chapter on St. Louis, as indicated by the citations, is based primarily upon the previous work of historian Charles L. Peterson. The work on Los Angeles draws upon Daniel Garr's dissertation, "Hispanic Colonial Settlement in California, Planning and Urban Development on the Frontier, 1769–1850." A long conclusion is comprised of essays on Spanish problems in colonization, on the presidios, on Monterey, on church boundary disputes, and population and race, all in California. In some form or another substantial parts of the book have been previously published. Throughout, the book is well illustrated with reproductions of maps, drawings, and photographs.

While this book may present a collection of essays useful to students, it presents little, if any, new material to scholars of our Southwestern cultural heritage, and it suffers from a lack of balance. In spite of an ambitious title, little mention is made of Florida and Texas—San Antonio, which along with Santa Fe is one of America's unique cities with Hispanic roots, is not even mentioned. No effort is made to acknowledge ranches, some of which became towns. Stylistic inconsistency further detracts from the work.

With landscape as a focus, various aspects of American culture and their influence upon the ordering of our environment are presented by John R. Stilgoe. The study includes spatial ordering traditions brought to the new land by Spanish colonists in the Southwest and by the English in New England, the Tidewater, and Piedmont regions. It surveys not only such landscape features as roads and gardens but also the influence of various works such as lighthouses, farmsteads, cowpens, and graveyards. In addition, the influence of rural churches, schoolhouses, and fairs is included. The speculative circumstances underlying the founding of countless American towns and villages are also surveyed. Some aspects of the history of technology are brought in with surveys of furnaces, mills, sawmills, and factories.

In recent years scholars have been giving increased attention to common, or folk, culture—that is, culture based upon traditions and customs passed from one generation to the next and "understood and agreed upon by all" (p. ix). In addition, ethnic material culture has occupied the attention of many, adding to understanding of our diverse and interesting heritage in landscape, communities, and architecture.

Throughout, descriptions of architecture are included and related to the landscape. For example,

Spanish building traditions are discussed using the town of Chimayo, one of the most charming in the Southwest, as a representation (pp. 34–42). American log culture is also discussed (pp. 177–80). Farmhouses, surely a subject for an extensive treatise, are likewise included.

The book is well researched, informative, and interesting. Well written, it brings to light many important aspects of folk culture. Better and more extensive illustrations, however, would have improved it; the few graphics that are included are often spoiled by poor cropping or lack of clarity. Further, the index is minimal, thus limiting its convenient use as a reference tool.

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FREDERIC COBLE JAHNER. *The Urban Establishment: Upper Strata in Boston, New York, Charleston, Chicago, and Los Angeles*. Urbana: University of Illinois Press. 1982. Pp. xi, 777. Cloth \$35.00, paper \$17.50.

Books bear the mark of their origins, and *The Urban Establishment* seems to provide an exceptionally clear example of this rule. While planning a study of the Boston Brahmins, Frederic Cople Jahner tells us in his preface, he asked a former teacher whether a comparative study of several urban elites "might not be more significant than an examination of a single upper class." Of course, he was advised to expand the study. After many years of work, he now offers us a book that consists of monograph-length chapters on Boston, New York, Charleston, Chicago, and Los Angeles.

Had Jahner approached the matter of comparative history a bit differently, the book would have been much better. Important scholarship proceeds from a significant question—one generated either by previous scholarly discourse or, in rarer instances, one completely novel with the individual historian. Jahner had no question to ask of Boston—or any of the other cities. Instead of turning to comparative history out of a concern that the question he wished to answer in respect to Boston could not be answered without comparison, he seems to have proliferated comparisons to mask fuzziness of purpose. Indeed, when one gets to the epilogue of this book, what one finds out is not, as one might have reasonably hoped, a statement of the issues illuminated by the book and their significance. No, one finds more comparisons—this time from the European past. Instead of research in the service of thought, I fear we have here a case of research in place of thought. A book of prodigious research and of wide awareness of sociological commentary on elites adds up, after more than seven hundred pages, to very little.

It is earnest, honest, and workful, but it is unimaginative in form, lacks a compelling question, and is, ultimately, rather insignificant.

Jahner seems to be most interested in measuring the persistence of patrician domination of the economic, political, and cultural life of cities. He finds a general decline, though it is uneven, with persistence highest in Boston and Charleston. Such conclusions are hardly novel, but he seeks to give them quantitative precision. In this he is partially successful, and he raises some hard questions about Edward Pessen's statistical findings on New York and Boston. But since he does not print lists of his own group of patricians, I am not sure we can be any more confident of his classifications than we are of Pessen's. It is difficult, especially over long periods of time, to manage a statistical distinction between a patrician and a rich person. Indeed, even in a simple case, Jahner seems to be inconsistent. On page 233 he treats Philip Hone of New York as a parvenu, and then four pages later Hone seems to have become a patrician. This blurring of categories is disturbing in general, but particularly so when the theme of the book is the gradual replacement of the patriciate by new wealth.

In this study Jahner seems not at all interested, at least not in the analytical way that E. Digby Baltzell, Sam Bass Warner, and others have been, in the relation of elite status and class formation to place. There is no systematic inquiry into the interplay of national or societal development and city development. The diverse data that one finds in these pages seem to suggest at once the nationalization of an upper class and the simultaneous emergence, out of the patriciate eclipsed nationally, of a self-consciously civic elite. But Jahner offers no direct guidance here.

Giving evidence of the behaviorist orientation of the profession during the years when he wrote this book, Jahner seeks and offers very little insight into the minds, intentions, and general culture of the patriciate. Nor does he seek to grasp the interconnections between the cultural, political, and economic realms; he treats each separately. What he presents in abundant detail is an external catalogue of dollars earned, offices held, and formal associations joined. Out of this process a judgment of failure gradually emerges. He leaves us, however, far from understanding the culture, the power, and the significance of American urban elites. That Jahner's big book does not succeed must not, however, daunt us. It is possible to engage these issues fruitfully. Indeed, the way has been charted, I think, by Peter Dobkin Hall in his provocative and theoretically sophisticated book, *The Organization of American Culture, 1700–1900* (1982).

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JEROME M. CLUBB *et al.*, editors. *Analyzing Electoral History: A Guide to the Study of American Voter Behavior*. (Sage Focus Editions, number 33.) Beverly Hills: Sage. 1981. Pp. 310. Cloth \$20.00, paper \$9.95.

PAUL KLEPPNER *et al.* *The Evolution of American Electoral Systems*. (Contributions in American History, number 95.) Westport, Conn.: Greenwood Press. 1981. Pp. xiii, 279.

These two books reflect different facets of the quantitative analysis of popular voting behavior, one substantive, the other primarily methodological. Over the past twenty years such analysis has reached maturity in many monographs, which have delineated voting patterns, classified them, and developed these findings into a new understanding of the American political process. Both of these books mirror that mature state, successfully summarizing the state of the art and helping clear the decks for further efforts.

The authors of the essays in the volume edited by Paul Kleppner, all of whom have been among the vanguard of these research efforts, present a thorough and fresh understanding of the nature of American popular voting and its consequences for the rest of the political universe. They spell out the results of their own and others' quantitative research on voter turnout, the distribution of the vote, and its socioeconomic basis—plus investigations of party development and larger issues of political growth. They focus on integrating the whole into an understanding of the internal dynamics of the system over time following the five party system organizing scheme articulated by William N. Chambers and Walter Dean Burnham in 1966 and extensively used since. In keeping with that dominant paradigm, the authors describe a pattern of long periods of voter stability regularly interrupted by short bursts of critical realigning behavior to form five, or possibly six, successive party systems. Each of them successfully illustrates that conception with in-depth studies of the nature of voting coalitions within, and the formative influences on each of the party systems. They go beyond that to articulate the other elements of the political process, most particularly the nature of party structure and operation. In all of this there is a search for precision, clear definitions, and thorough stocktaking of the political process in all its systemic regularity. The final essay, by Samuel P. Hays, while accepting the main lines of the others' approach, goes a step beyond to criticize too exclusive a reliance upon voting studies as the fulcrum of political history. Hays believes such a reliance leaves out too much about power and policy, alternative political activities outside of the voting booth, and party organizations created to deal with voters.

Hay's criticism broaches one aspect of the prob-

lem posed by these essays. In fact, a number of historians have recently questioned the universal applicability of the critical election, party system organizing model because of its loose ends and many untidy elements. Some have sought alternative schema, while defenders of the idea have gone back to the drawing board to redefine, add to, and try to strengthen its elements. The essays here underscore some of the difficulties. The authors are well aware of the problems and are very cautious as a result. Ronald Formisano does not find a first party system, although protoparties existed before 1815. William Shade begins his discussion of the "second" party system in 1815, but his real focus is on the period after 1836. Having rid ourselves of the first fifty years under the Constitution is not helpful to the model being used. Richard Jensen's essay on the last party system, that since 1932, reports on a decaying and complicated situation that has gone on for so long without realignment as to raise questions whether it really fits the larger dynamic suggested. Only Shade's, Paul Kleppner's, and Walter Dean Burnham's material—spanning the years between the late 1830s and the New Deal—comfortably fits the paradigm under review. In short, the party-systems conception rooted in intense and extensive *partisan* voter commitment and the institutions structuring that, apparently applies to only about half of the American political experience under the Constitution. Obviously, there is a major intellectual problem still to be worked on in regard to perceiving the shape and dynamics of the American political universe at different moments. When that work is undertaken, it will have this book as the model presentation of what we have already learned and as a springboard for continued efforts.

These new research efforts will also be immensely aided by the materials in *Analyzing American Voting*, edited by Jerome M. Clubb. Its authors, too, have been in the forefront of the enterprise of locating and systematically analyzing quantitative historical data by statistical means. Their essays deal with the sources of election returns, the organization of that evidence for analysis, and the method of conducting the analysis itself. They argue strongly for the use of relatively high-powered statistics as the best way to squeeze the fullest and most accurate understanding from electoral and social data. They make cautious assessments of the difficulties historical researchers face in this field, given the problems of clearly specifying the units of analysis desired and the difficulties presented by some of the data available. Their statements are well organized and are presented in such a way that anyone pursuing this kind of research will understand what has to be done and what the opportunities and problems are. In addition, two of the essays make important substantive

contributions to historical knowledge. Howard and Kay Allen's piece on corruption at the polls is an exhaustive and important analysis that questions our customary belief that there was widespread chicanery in the voting process. Ray Shortridge's intriguing article challenges the accuracy of the population census in the nineteenth century and suggests that, as a result of census error, turnout at the polls may not have been as high as we have assumed. Finally, there is, in these pages, also a hidden agenda, reflecting the position of the writers on a number of controversies among electoral analysts about similarities and differences between the nineteenth-century voting universe and more contemporary ones.

Not all of the judgments made will convince other scholars. Some analysts have raised interesting methodological questions about using very high-powered statistics on some electoral data. A number of the conclusions in Shortridge's piece also rely on maximizing the possibilities of the errors made, so that further study is needed before his conclusions can be accepted.

Nevertheless, this volume and Kleppner's as well are exemplary in their exposition, stocktaking, agenda setting, and suggesting how to get on with the job. They reveal that we have reached an intellectual plateau in this exciting enterprise of studying the dynamics of American voting behavior and its consequences. They effectively describe how the underbrush has been cleared, how the trails have been marked, and what vistas are still to be explored. Political historians will greatly benefit from both works, while at the same time looking forward to further exploration of the many remaining loose ends and controversial matters that the books underline.

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DAVID T. COURTWRIGHT. *Dark Paradise: Opiate Addiction in America before 1940*. Cambridge: Harvard University Press. 1982. Pp. 270. \$20.00.

H. WAYNE MORGAN. *Drugs in America: A Social History, 1800–1980*. Syracuse, N.Y.: Syracuse University Press. 1981. Pp. xi, 233. \$20.00.

There is a good deal in both of these books that is familiar (particularly to those who know David Musto's *The American Disease*) and a good deal in them that is overlapping. Yet each has a distinct character and each is a very significant contribution to the literature. Neither, it might be noted, is really a very large book: the actual text of H. Wayne Morgan's book covers 167 pages and of David T. Courtwright's book 147 pages. The remainder in

each book consists of voluminous and interesting notes, bibliography, and index.

Courtwright stops his study at 1940 for a variety of reasons, but mainly because he believes the story after 1940 to be so complicated and so controversial that it requires a separate study. Undoubtedly such an extended study is desirable, but Morgan's final chapter does offer a succinct and valuable coverage of the period after 1950. Morgan pays considerable attention to chloral hydrate, cannabis (marijuana), and cocaine, as well as to opium, morphine, and heroin. Courtwright has restricted himself largely to the last three addictive narcotics; the others, that may cause dependence but are not addictive, he discusses only *en passant*.

The two books present a contrast in methodology: they are examples of two generations of historical writing. Morgan's approach is that of narrative history, sometimes even a bit anecdotal. It is based on meticulous and thorough study and assimilation of a vast literature, adroitly analyzed and brought together. Certainly one who has this acquaintance with the sources can be allowed the generalizations that he makes along the way. Courtwright, on the other hand, although he has made just as good use of what he calls the "traditional historical approach" and has carefully surveyed the sources, presents a study based in large part on quantification and statistical data. He does describe and interpret the events, but his major thrust is to study what went on by looking at the numbers. His book is thus rather replete with tables, graphs, and charts.

The history of addiction that both books outline is straightforward enough. Opium and morphine addiction in the United States in the nineteenth century was largely iatrogenic (physician induced) and the typical addict was a "respectable" upper-middle-class woman in her middle years. (Both authors are aware of the addiction of working-class mothers and their children in Great Britain, but the possibility that the same thing was going on in the United States, or if it did not, warrants further exploration. Courtwright recognizes that evidence of a "statistical nature is scant," and he cites a survey—1888–89—that showed that 16.3 percent of the prescriptions in a drug store with an upper-class clientele called for opium or morphine, while only 10.7 to 12 percent of prescriptions in drug stores patronized by "poorer people" called for these narcotics. This is hardly impressive: the poorer people were more likely to go to a dispensing physician, or avoid a physician altogether and dose themselves with or without the advice of a pharmacist, or resort to patent medicines. Their use of narcotics was probably much less reflected in prescription records than that of the wealthier patient.) Iatrogenic addiction in the nineteenth century was speeded up with the introduction of the hypodermic syringe, but by the 1870s

physicians were aware of the addictive power of opium and its derivatives. The Civil War did not figure as prominently in the spread of addiction as is commonly supposed.

Addiction was not all attributed to therapeutic origins. As early as the 1840s addiction for "excitement and inebriation" was evident, and after 1870 white Americans picked up opium smoking from the Chinese immigrants. The growth of such addiction, the decline in iatrogenic addiction as medical science and practice improved, and the introduction of cannabis in the 1840s and cocaine in the 1880s meant that at the beginning of this century the addict population had changed. The typical addict had become a young, urban male. Addiction became increasingly connected with crime, prostitution, and the "fringes" of society. Heroin, which had been introduced by physicians at the turn of the century, became "the illicit opiate par excellence" (Courtwright) and spread throughout the country in the 1920s and 1930s. By 1950 (Morgan) the typical addict was a young, black male from an urban ghetto, and other teenagers were experimenting with drugs as well. By the 1970s (Morgan) two decades of counterculture involvement with the drug scene and the introduction of psychotropic drugs found many substances popular with a large range of people. Morgan has found some "maturity" in drug use but concedes that an evil "genie remains loose."

There is much more to be found in these books: for example, the changing explanations of the addiction process, the reaction of society to the addict (Courtwright makes the cogent observation that what society thinks about addiction depends on who is addicted), the medical groping for cures, the treatment and maintenance of the addict, the regulation of narcotics, marijuana, and cocaine and its impact, and the racial and xenophobic attitudes involved in the drug scene.

Several aspects of the Courtwright book deserve special comment. Most important is his disagreement with the contention that the effect of the enforcement of the Harrison Narcotic Act of 1914 was the criminalization of the American addict. This contention has had important implications for public policy, for it served as the rationale for methadone maintenance and it suggests a switch back to cheap and legal drugs. Courtwright argues that the changes in the American addict took place before rather than after the passage of the act: the changes had taken place between 1895 and 1914 essentially because of changes in medical practice and the physician's new reluctance to prescribe narcotics. All of which may be true, but just because criminalization had begun before the Harrison Act was passed, it does not follow that the act did or did not add to the criminalization process.

Courtwright's thesis here is to some extent dependent upon his being able to demonstrate that opiate addiction was actually declining before 1914. He presents four types of statistical evidence to this end: surveys of physicians' and pharmacists' records, records of maintenance programs, military medical examination records, and opiate import statistics. Each of these is presented despite the acknowledgement that each "has its limitations." Indeed, only one of the four, the opiate imports, comes close to being convincing. By a really clever use of these import statistics Courtwright is able to devise a chart that does indicate a decline in addiction.

When, however, Courtwright seeks to determine the number of addicts in the United States, the best his quantification can do is to suggest a maximum number. Thus, taking one compiler's extrapolation from figures of a 1924 survey of maintenance programs (one of the types of statistics with even more limitations than Courtwright acknowledges) that indicated 104,933 addicts, Courtwright assumed that with 100 percent underreporting the maximum number of addicts was 209,866. What is not explained is the logic by which one suspect number becomes less suspect by doubling it. The number of addicts after 1900 and before 1914, Courtwright finds it "safe to assume," was 313,000, of which 221,559 were addicts and the remainder opium smokers. The figure 221,559 is derived from import statistics, but it does not take into account, among the factors that Courtwright himself notes, the 35 percent of the medical imports that were wasted or went to nonaddicts. Clearly the 221,559 maximum was a safe assumption, but it does not tell how many addicts there actually were. Even quantification has its limits.

But there is ample evidence, from Morgan as well as Courtwright, that American narcotic laws (1914 and after) were passed, interpreted, and defended "on the basis of misleading, even fraudulent information." To procure passage of legislation, inflated figures of addiction were proffered; to show that the bureaucracy was successful, low levels of addiction were later presented. Courtwright examined the reports on which such figures were based and concluded that it was impossible to infer any pronounced decline in the number of addicts up to 1940.

The "American Disease" is neither new nor one that lends itself to a simple therapy.

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IVOR NOËL HUME. *Martin's Hundred*. New York: Alfred A. Knopf. 1982. Pp. xx, 343. \$17.95.

The nucleus of Martin's Hundred, a 1616 land grant of the Virginia Company of London below Jamestown on the James River, was Wolstenholme Towne, a lightly paled and fortified enclosure with a few post-in-ground domestic and farm structures inside and outlying. Discovery of the site of this tiny settlement, seven miles from Williamsburg, and the evidence revealed of the Indian massacre of 1622, is the subject of a notable book.

Expressly, *Martin's Hundred* is the story of how Noël Hume, director of archaeology for Colonial Williamsburg, and his wife Audrey, also an English-born archaeologist and writer, found the site inadvertently while following administrative direction to test the bottoms between the terrace on which Carter's Grove mansion is perched and the James River. The idea was to enable CW, which operates Carter's Grove, to demonstrate the function of a typical eighteenth-century plantation, complete with slave quarters, craft shops, barns and all, for the edification of the visitor. The last thing the planners wanted was the distraction of a seventeenth-century story, which could conceivably overshadow that of the Carter mansion. Alas, the fact was that Carter's Grove was anything but a typical plantation, and it was quite free from the encroachment of that picturesque plantation bustle, which the builder had deliberately set about to escape. So much for a fine idea.

At first Hume thought he had found traces of a nearby early pottery kiln, and managed to interest *National Geographic* in funding the search for it, only to find the kiln was not there either. When he did find Wolstenholme Towne with the continued support of *National Geographic*, he made the best of things by producing a lavishly illustrated article in the June 1979 issue. Taking opportunity a step further, the archaeologist used artwork from the article to enhance a book entertaining enough to be chosen an alternative offering by the Book-of-the-Month Club.

The significance of these findings lies in additional confirmation of a practice recorded at Jamestown, the building of primitive, post-in-ground ("earthfast") houses, this time illustrating definitively the bawn-type palisaded enclosure after the practice in northern Ireland during the contemporaneous British incursion. A vivid garnishment to the picture at Martin's Hundred was the discovery of the massacred dead and their already hopelessly obsolete armament. For those who like their scholarship straight, the building practice of settlers whose lives and houses were alike of brief duration has been admirably documented archaeologically by Cary Carson, and others, in a definitive article "Impermanent Architecture in the Southern American Colonies" in *Winterthur Portfolio* (1981)—too late to be cited here.

Readers unhappy with popularization may be comforted. The research for *Martin's Hundred* is exhaustively done in order to interpret specific evidence in the ground and is expertly revealed, conserved, and analyzed. The author is completing a formal, scholarly monograph that will, one hopes, be the first of a series long overdue from Colonial Williamsburg on its archaeological investigations.

A caveat. Early Jamestown of the Wolstenholme era did not disappear, as Hume suggests, with the erosion of the 1607 triangular First Fort into the James River together with a portion of the southern shore of Jamestown Island. Nor was there a "dark age" of seventeenth-century evidence there or elsewhere in Virginia. The sixty-one burials found forgotten beneath the foundations of the 1666 Ludwell row house on the first high ground beyond the fort site may well be those who perished in the fort during Starving Time, 1609–10. Following their rescue by Lord Delaware in 1612, the settlers rapidly expanded out of their fort as did the plantation bawn builders. Traces of at least one post-in-ground house have been excavated, and 127 other structures of all kinds.

After the massacres of 1622 and 1644, which Jamestown escaped, the persistent settlers discarded their useless armor and an alarming number of spirit bottles, busied themselves with hospitality to the Burgesses, fires, and rebellion (leaving ample archaeological evidence), until the capital moved to Williamsburg in 1700. This the reviewer has recorded in *Archeological Excavations at Jamestown, Virginia* (1958).

Mistakes are minor—1916 for a seventeenth-century date, and Kieth Egloff, archaeologist, suffers an extra "g" in his name. There is a useful index, brief bibliography, and references by chapter at the end of the book.

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DAVID W. GALENSON. *White Servitude in Colonial America: An Economic Analysis*. New York: Cambridge University Press. 1981. Pp. xii, 291. \$29.50.

David W. Galenson has written a superb book about colonial American labor, and more specifically about white indentured servants and their eventual displacement by slave labor. Yet, this is a book that suffers a certain indecision on its audience and purpose. Roughly one-half the book is addressed to the historian, and it offers a lucid account of the men and women who, during the seventeenth and eighteenth centuries, contracted several years of their labor in exchange for transportation to the British West Indies or the mainland. The second

half of the book speaks to a second audience, the economists. Here, the volume offers an abstract discourse in the microeconomics of labor markets. Historians will find the first half of Galenson's book extraordinarily useful for its thorough discussion of servants' origins and destinations; as for the economic theory explaining the transformation from white servants to slavery, historians will find it difficult but rewarding reading. Galenson's thesis is an important contribution to a controversial issue that, I will argue, he does not fully resolve.

Galenson is to be commended for the first half of his book. It provides perhaps the most thorough discussion of indentured servants that we are likely to have for some time. Using a sample of surviving English registrations, Galenson tells a great deal about the third of a million people who came as servants from Britain to Anglo-America. Most servants in both the seventeenth and eighteenth centuries were male and young, fifteen to twenty-five years being the average age. Although age and sex compositions seem not to have changed much during a century and a quarter, fundamental change did occur in servant skill levels. Before 1700, Galenson reports that servants came almost equally from four groups: artisans, farmers, laborers, and youth not yet placed in the occupational structure. On this highly controversial issue of servant origins, the author's discussion is sensible, cautious, and sufficiently persuasive as to constitute a standard view of pre-1700 servant migration. Contrary to Mildred Campbell's view, seventeenth-century servants seem to have consisted of a large contingent of unskilled migrants.

After 1700, servant skill levels rose appreciably according to Galenson. Moreover, rising skill levels appeared first in the West Indies and later on the southern mainland. Given these patterns, Galenson presents a three-stage model of the transformation of the colonial labor force: first, white servants did both skilled and field work; second, slaves displaced whites in unskilled crop production; third, creole slaves, having acquired artisan skills, displaced the residual skilled servants. The West Indies had arrived at the third stage by the Revolution while the Chesapeake stood between the second and third stages. In Galenson's view, the rising skill levels of the servant trade reflected a labor market adjustment to slave substitution.

The second part of the book introduces sophisticated economic theory to explain servant migration and the transformation of the labor market. I fear that many historians will be disinclined to follow the author since deductive theory abounds, evidence is slim, and the assumptions are heroic. A few guideposts may be helpful. Galenson first argues that the servant trade was conducted in competitive markets

and consequently that servants voluntarily entered into contracts and that the economic outcome was nonexploitive. Historians have good reason for skepticism on all these points. Even more problematic is Galenson's major interpretation of the transformation of the American bond labor force. He attributes the three-stage shift to changes in relative prices and to supply elasticities. As servants became more costly, planters replaced them with field slaves; their descendants acquired skills that undermined the skilled servant migration in the eighteenth century.

That such a provocative economic interpretation of slavery could be advanced with scarcely a shred of price evidence almost defies credulity. One body of price data from the Chesapeake is consistent with Galenson's thesis that planters rationally chose slaves over servants at the end of the seventeenth century; but that same evidence indicates that when relative prices favored servants after 1720, planters irrationally persisted using slave labor. The inability of Galenson's price model in explaining the retention of slavery after 1720 thus implies that his model has not identified the sufficient cause of initial transformation in the 1680s. Perhaps the once-and-for-all change to slavery has as much to do with Edmund Morgan's thesis that servants had become a social problem or perhaps it reflects agroecological changes favoring slaves over servants. Economic theory, despite its elegance, evades the issue.

The irony of Galenson's generally intelligent book is that when evidence does not fit theory, he wriggles free with the bland noneconomic assertion that maybe eighteenth-century Englishmen did not want to work in the fields alongside slaves. Such easy capitulation is disappointing to those historians less skilled than Galenson in economics yet more steadfast than he to an economic interpretation of the American past.

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JERE R. DANIELL. *Colonial New Hampshire: A History.* (A History of the American Colonies.) Millwood, N.Y.: KTO Press. 1981. Pp. xvi, 279.

This concise and well-wrought study is the first comprehensive history of colonial New Hampshire since Jeremy Belknap's pioneering three-volume work was published nearly two hundred years ago between 1784 and 1792. Like Belknap's work and previous volumes published in the "History of American Colonies" series of which it is a part, Jere R. Daniell's book is mostly a narrative of the main political events and developments. He traces

the colony from its first settlement in the 1620s through its gradual amalgamation with Massachusetts during the mid-seventeenth century, its organization as a separate royal colony in 1679, its tortuous but ultimately successful adjustment to royal government between 1680 and 1715, its achievement of a stable political order over the following half-century, its participation in the intercolonial wars between 1689 and 1763, and its relatively passive response to the crisis in Anglo-American affairs between 1763 and 1774 when it then moved abruptly to armed resistance and independence. Though Daniell's account is often analytically considerably richer than that of his predecessor, it is striking testimony both to Belknap's sophistication and to the extensiveness of his research that Daniell offers surprisingly few new details and no major revisions of Belknap's basic political narrative.

Where Daniell adds enormously to Belknap is in his attention to New Hampshire's socioeconomic development. In keeping with modern sensibilities, Daniell devotes almost a third of his book to this subject. Settled primarily by emigrants from southern New England, New Hampshire to some degree displayed the emphasis upon family, church, and community usually associated with Massachusetts and Connecticut. Yet, in many ways, the New Hampshire experience deviated sharply from those of the older orthodox Puritan colonies. Not only did it develop much more slowly: after nearly nine decades of settlement, it contained only six towns and fewer than ten thousand people, far fewer than Massachusetts could boast at the end of its first decade! Similarly, despite the gradual assimilation of the four original towns to Massachusetts patterns of socioreligious and political organization while they were attached to that colony prior to 1679, New Hampshire and its towns were always more diverse, more flexible, and less inclined "to accept the degree of social control exercised in communities elsewhere in New England" (p. 56) than towns apparently were in the two older colonies. For that reason, perhaps, New Hampshire's rapid territorial, demographic, and economic growth between 1715 and 1775 and the emergence of a more complex society as a result of that growth seem to have produced considerably less disruption than did similar developments in Massachusetts and Connecticut. Indeed, New Hampshire during these years enjoyed a degree of political tranquillity that was without contemporary parallel in the region.

By thus calling attention to the distinctiveness of New Hampshire's colonial development, Daniell has moved considerably beyond his original intention of providing an up-to-date history of the colony and has presented us with still another vivid reminder of the rich variations in the early modern Anglo-

American colonizing experience, even within allegedly homogeneous cultural regions such as New England.

JACK P. GREENE
Johns Hopkins University

LAUREL THATCHER ULRICH. *Good Wives: Image and Reality in the Lives of Women in Northern New England, 1650-1750*. New York: Alfred A. Knopf. 1982. Pp. xv, 296. \$16.95.

Laurel Thatcher Ulrich's book eliminates the excuse that historians too often call on to justify their neglect of women's lives in colonial America: that the necessary documents do not exist. Sensitively and ingeniously combing through household inventories, account books, court files, indentures, captivity narratives, and men's diaries, Ulrich reveals the multifaceted richness and reality of women's roles, and lets the modern reader hear women's voices speaking from their oral culture, despite the lack of sources written in women's hands. As were her subjects, Ulrich is extraordinarily steeped in the homely details of colonial life; she shows an authentic understanding of colonial society, communicating the multiple paradoxes of its coincident patriarchal, hierarchical, and communal values, its religiosity and earthiness.

The complexity of women's ordinary roles and the reaches of their extraordinary ones interest Ulrich. She likes to make these different roles discrete: for example, in the norm, housewife, mistress, consort, mother, neighbor, and so forth; in extremity, virago, heroine. Her identification of a married woman's role as "deputy husband" is particularly useful, explaining the paradox that women could typically cross gender boundaries without upsetting the patriarchal order, so long as they were acting in the service of family loyalty. Historians' confusion between women's performance of "male" types of work (for example, running a business or a farm) and independence or individual opportunities for women, is thus cleared away, as Ulrich reminds us that *position* was always more important than *task* in premodern society.

Whether premeditated or not, Ulrich's method of searching in contextual documents for hints and clues to women's lives has the worthy consequence of revealing women's lives and actions *in the social order*. As much as the study evokes almost palpably women's daily household chores, and the omnipresence of pregnancy, lactation, child-nurture, and children's deaths, it also forcefully presents women as neighbors, traders, busybodies, rumor-mongers, sources of political or religious pressure, community saviors, members both of a community of women

and of a larger community of men, women, and children. Even the chapter on religion focuses not on women's doctrinal proclivities but on their roles in religious establishment, the longevity of ministers, and the creation of new congregations by "outliers." Ulrich refuses to abstract "women's status" or to compare the colonial era in this respect to later centuries, believing that the many facets and internal shifts in women's status are too complex to allow it, but her portrayal of women's exercise of power through language—spoken, not written language—is most thought-provoking on this score.

Ulrich alludes to the social and economic change that increased trade and population density brought during the colonial period and notices altered childbirth practices and new models for female behavior by the mid-eighteenth century. Unlike many colonial historians, however, Ulrich does not seem fascinated with the puzzle of modernization. Unaccompanied by investigation of reasons or engines for change within women's own lives, her references to change tend to make women appear cast about by shifting currents, far from shaping their own destinies. Perhaps this implication should be discounted as merely a consequence of the author's definition of her scope, however, for it contradicts the accomplishment of the study as a whole, which is to place women in the thick of social history.

NANCY F. COTT
Yale University

DOUGLAS LAMAR JONES. *Village and Seaport: Migration and Society in Eighteenth-Century Massachusetts*. Hanover, N.H.: University Press of New England, for Tufts University. 1981. Pp. xx, 167. \$15.00.

In this brief, narrowly focused study, Douglas Lamar Jones examines quantitatively the role of migration in eighteenth-century Massachusetts. In pushing migration studies back to the colonial period, Jones demonstrates that long before the nineteenth and twentieth centuries, changing economic conditions forced Americans to move west in search of new land or to emerging commercial centers in a quest for jobs. In long-settled communities, the pressures of population against the land supply forced migration for economic survival. While this and his other conclusions are not strikingly new, his research forcefully confirms the earlier but sketchy findings of Kenneth Lockridge, Philip Greven, Daniel Smith, Linda Bissel, and Robert Gross.

In his assessment of the social impact of rapid population growth on colonial New England, Jones compares two neighboring towns. One, Beverly, was an expanding seaport near Salem, while the other, Wenham, was a small inland farming community. One represented the possibilities of commercial

expansion for adapting to population growth and migration; the other symbolized the limits of agricultural development and the ultimate reliance on outmigration to balance population growth and diminishing resources. The two divergent communities provided representative situations for analyzing the role of migration in early American society.

In comparing and contrasting the way these towns coped with their demographic problem, Jones relies chiefly on data from tax lists and family reconstitutions derived from vital records and genealogies. He uses the computer to study systematically such topics as population turnover, economic mobility, family formation, and patterns of inheritance. His computer-assisted analysis relies heavily on the use of the sophisticated statistical procedure known as analysis of variance. This allows him to measure simultaneously the causal relationships among several variables. The results prove especially useful in comparing the impact of migration on economic opportunity, inheritance patterns, and age of marriage in the two towns.

Jones argues convincingly that the eighteenth century represents a transition period between the comparatively low levels of mobility in the seventeenth and the higher levels of the past two centuries. The increased mobility of the eighteenth century represents the beginnings of a "double safety valve" that would become so prominent in modern America: the movement to acquire new lands or to find new opportunities in commercial or industrial centers. More debatable is his conclusion that the process resulted not in communal decline but in a new social stability. For Jones, "the eighteenth century marked the beginning of a long process in America when internal migration successfully reallocated people and economic resources. As a result, it realigned the patterns of social stability" (p. 119). Quantitative analysis does not always provide a sound basis for determining the quality of change or the value people place on that change.

This is not a complete study of the impact of migration on colonial society, for Jones says nothing about the influence of migration on social and political leadership within the towns he examines. Also, he fails to measure the effects, long or short term, of war on the migration process. By clearly distinguishing, however, between a town's migrants and permanent residents and devising a methodology for analyzing their changing economic ranking over time, this book gives us a sound basis for further work on migration in early America.

WILLIAM F. WILLINGHAM
Lewis and Clark College

EUGENE R. SHERIDAN. *Lewis Morris, 1671–1746: A Study in Early American Politics*. (New York State

Study.) Syracuse, N.Y.: Syracuse University Press. 1981. Pp. xii, 257. \$20.00.

Lewis Morris is one of those intriguing personalities who strides across the pages of the history of colonial New York and New Jersey like a Brobdingnagian figure. Long recognized as one of the most prominent political leaders of both provinces, the exact nature of his importance and the essence of his contribution to the political development of the colonies have still eluded the interpretive grasp of historians. The diversity of Morris's activities, remarkable even in an age that produced many men of multiple accomplishments, may account for the historiographical difficulty. The son of an English military officer and Westchester landowner, Morris received little formal education yet rose to the position of chief justice of the New York Supreme Court and became one of that colony's most effective political pamphleteers and coauthor of the first play written and published in America. As for his legal knowledge, William Smith, the contemporary historian of New York, conceded that Morris had no equal. At the age of nineteen, he inherited not only his father's Westchester lands—later designated a manor—but also the much larger properties of his uncle: a sixty-two-hundred-acre estate and an iron works in New Jersey, another large tract on Long Island, and a force of sixty-six slaves. The whole made him one of the wealthiest men in both colonies at the beginning of the eighteenth century.

Morris's political career was similarly fruitful. He was appointed to the New Jersey council, served as legislative leader of the assembly both in New York and New Jersey at different times, and ultimately was named governor of New Jersey. An elusive politician, Morris appeared variously as a flaming leader of the popular cause against the executive and as a staunch supporter of gubernatorial prerogative. Perhaps his chief fame as a "popular" spokesman came from his opposition to Governor William Cosby in the controversy that led to the noted Zenger trial in 1735. Morris's position in this contest seemed to make him the champion of a free press, assembly rights, and an independent judiciary, yet even Eugene R. Sheridan, the author of this competently written biography, concedes that Morris's personal ideology was always elitist: he believed that the landed aristocracy should control the government. Not unwilling to enlist small farmers and urban artisans in his "party," he nevertheless viewed them as the "scum and dregs of mankind" and the "Rascality" when they opposed his actions. When he assumed the governorship of New Jersey in 1738, he dealt more arbitrarily with his assembly than any of his predecessors, and when he died eight years later, he was so universally detested that no assem-

blyman would vote to grant his heirs his arrears of salary.

Sheridan's biography draws on traditional sources for what is essentially a political portrait and a public life. There is little here on Morris's management of his estates or his slaves, his family or intellectual interests—except as they affected his political career, and not much on the social context of politics in either New York or New Jersey. Sheridan sees some significance in Morris's career other than an admittedly remarkable penchant for mendacity and tergiversation: his fluctuating roles as popular champion and prerogative man suggest to Sheridan that colonial Americans drew on "court" and "country" ideology alternatively as it suited their interests, and Morris helped to explicate and refine each position in the course of his changing political course. Sheridan concedes that Morris was a "consummate opportunist" and a "trimmer" throughout his life but insists that Morris's erratic political behavior denotes more than a desire to satisfy personal ambitions. Sheridan views Morris as a paradoxical figure who helped to enunciate both the royal and the popular positions that were to figure so significantly in the later Revolutionary controversies. What Sheridan dignifies as paradox, however, may be no more than incongruity at best or hypocrisy at worst.

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EDMUND BERKELEY and DOROTHY SMITH BERKELEY. *The Life and Travels of John Bartram: From Lake Ontario to the River St. John.* (A Florida State University Book.) Tallahassee: University Presses of Florida. 1982. Pp. xv, 376. \$25.00.

John Bartram (1699–1777) of Pennsylvania has been one of the most difficult of the colonial American nature reporters for biographers to capture. It is not that his works are obscure or motives puzzling, quite the opposite. He set out to contribute to the new nature project exciting the eighteenth century and won wide recognition for his efforts: praise by Linnaeus, regard by the Royal Society of London, a wide correspondence with the leading virtuosi of his day, appointment as "His Majesty's Botanist for North America," appointment to the Royal Academy of Sciences in Stockholm, and "a species of eternity," as Peter Collinson called it (p. 67), by having his name attached to a genus of plants (*Bartramia*) by the botanical community.

He was so much of his time and caught up in the logic of its developing learning, so much set in the swirl of certainties and collapsing myths, that his discoveries and speculations yield up their richest meanings only to those with a feel for the era and a

good deal of background knowledge. To tell this story to the uninitiated takes a lot of explaining. The authors of *The Life and Travels of John Bartram* chose instead to speak to the well informed and already interested.

Edmund Berkeley and Dorothy Smith Berkeley are familiar to students of colonial American science as the coauthors of four earlier works on John Clayton (1963), Dr. Garden (1969), John Beckley (1973), and John Mitchell (1974). Their way is both old-fashioned and effective: they get the facts straight, verify them, trace down all sidetracks, correct errors of dating and attribution current in popular or prior works, draw from earlier, well-done works, search archives and distant libraries for morsels overlooked by others, work the whole into a coherent narrative, fully documented, including nine appendixes, and indexed for the easy use of readers who want to search out bits and, like themselves, track down leads.

The Life, then, is a definitive work of its sort. No one ever again should have to reconstruct Bartram's progress through the world. Reading those parts of his life I knew best from having worked over them myself, I found no flaws and learned a good deal. Dozens of times they glossed a name, added a detail I had not known, or supplied a context that had escaped me, so that I understood better. For all its matter-of-fact clarity, the book is moving at times. The Berkeleys' account of Collinson's death (p. 277) and the delay in telling Bartram, his long-time friend, accrues a kind of impact from the very plain telling. The events speak for themselves, it seems, and move us without the help of the prose. Such effects are no accident, any writer can testify.

The Berkeleys have served us well. Facets of Bartram's lively being remain to be understood, his dry, vernacular wit, for example, but they have built us an armature to hang a likeness on in time.

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Davis*

JURGEN HERBST. *From Crisis to Crisis: American College Government, 1636-1819*. Cambridge: Harvard University Press. 1982. Pp. xvi, 301. \$25.00.

The evolution of governmental, legal, and institutional aspects of American colleges and universities has received limited attention from educational historians, and Jurgen Herbst's welcome study attempts to rectify this shortcoming.

The book's initial section surveys the period from the genesis of colonial higher education to the founding of Queens (Rutgers) College in 1766. Here, Herbst traces the legal roots of colonial colleges to Reformation European concepts of degree-

granting institutions that were established as publicly funded corporations. The attempts, however, to govern early colonial colleges along Reformation principles proved difficult in America. Commencing with Harvard's 1692 charter, Herbst describes the various controversies that caused departures from Old World governing practices and a movement toward more adaptable public-private systems of collegiate control. The Great Awakening, with its emotionalism and questioning of authority is cited as the principal catalyst in this changing pattern. The Awakening brought challenges to existing colleges and led to the establishment of provincially oriented institutions that reflected the pluralistic composition of particular colonies.

The volume's latter portion, detailing the years from the Revolution to the early nineteenth century, depicts the extensive changes in American higher education. Colleges increased in number from nine to forty, and the pattern of single provincial colleges dissolved as rival state institutions were founded. New types of public and privately administered colleges were chartered by state legislatures; several states sought to establish their own higher educational systems; and the incorporation of professional studies shifted the traditional college curriculum toward the ideal of an American university. Controversy accompanied these developments as an increased distrust of "aristocratic" higher education brought new threats to the legal status of private colleges. From the actions of Pennsylvania's assembly in 1779 against the College of Philadelphia's charter, Herbst traces subsequent state moves to bring greater governmental authority over higher education. Finally, in the Dartmouth College decision, John Marshall formally repudiated the supposition that American colleges had been established as publicly funded civil corporations. In the Supreme Court's replacement of this concept with the English view of colleges as essentially nonpublic charitable corporations, Herbst sees the guarantee of an independent future for American private colleges.

Herbst's definitive study displays considerable expertise in legal and educational history. It is organized into short, concise, yet comprehensive chapters portraying general features of higher education in early America, and particularly the numerous controversies over collegiate control. The annotations display an extensive knowledge of American and European sources, and two useful appendixes illustrate the colleges established and the number of bachelor's degrees they awarded through 1820. Among minor criticisms, I believe it is more accurate to refer to Harvard, Yale, and Middlebury as Congregational, not Presbyterian colleges; Princeton graduates in 1749 did not "establish Augusta Academy" (p. 191) in Virginia; and in 1812, it was the

Federalists not the Republicans who "regained" control of Massachusetts's government (p. 213). Such secondary reservations do not detract from this very commendable work.

SHELDON S. COHEN
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JOHN PHILLIP REID. *In Defiance of the Law: The Standing-Army Controversy, the Two Constitutions, and the Coming of the American Revolution*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1981. Pp. viii, 287. \$20.00.

It is well known that American colonists inherited the long-lived English prejudice against standing armies and considered the use of troops to maintain order in the colonies, as in Boston in 1768, to be illegal and unconstitutional. John Phillip Reid examines the legal and constitutional side of this controversy. Colonists, he says, took the essentially seventeenth-century view that a permanent army threatened liberty while the British ministries of the 1760s adopted a more pragmatic view that justified a permanent, professional force both for defense and for internal police. As Reid sees it, the two sides represented sharply divergent interpretations of the constitution, hence the "Two Constitutions" of the title. Colonists shared with radical English Whigs and even with many parliamentarians the view that customary restraints limited government in its exercise of power. The ministry, on the other hand, had shifted to a new, relativistic concept of constitutionalism—an innovation that bespoke a belief in parliamentary supremacy. Whatever Parliament decreed was the law, and the constitution permitted it to command obedience, even, if necessary, through armed force.

Some of this is old; some, new. The American colonial adherence to seventeenth-century intellectual traditions is something that students of Revolutionary ideology are well acquainted with through the works of Caroline Robbins, Bernard Bailyn, and others. The other view, the one Reid associates with what he calls "the imperial ministry," is Reid's invention, perhaps more fairly his thesis, but it is a thesis that is not very convincingly documented. In fact, to the contrary, there is abundant evidence in this book that British officials on both sides of the Atlantic well knew how constitutionally limited they were in using troops for the purpose of maintaining colonial order. Put in slightly different focus, this certainly would be the book's major contribution, for the case seems overwhelming that the British scrupulously avoided using troops except when called upon to do so by local civil authority, and this was rare. Reid contends that a standing army subject to local, civil

control was a compromise between the "two constitutions." But the very notion that somehow a constitutional view entitling Parliament to use force could be compromised suggests that even the ministry understood the limitations imposed on it by customary restraint and constitutional tradition.

Perhaps the real question, however, is how much Reid's limited examination of legal and constitutional matters improves our understanding of the American Revolution. The answer is, not very much. Even legal and constitutional questions need context. Robbins and Bailyn place such questions in the larger realm of commonwealth or radical Whig ideology and, like a piece in a jigsaw puzzle, both the piece and the larger picture take on new meaning when seen together. The questions Reid asks are narrow and limiting; they avoid cultural and intellectual linkages.

The book suffers, moreover, from the author's almost perverse eccentricities. Reid's enigmatic chapter titles, his odd division of the world into "lawyers" and "nonlawyers," his sweeping attacks on other historians who stand defenseless as straw men, can variously be characterized as annoying, arrogant, and incredibly naive. The malapropisms and improper word choices, furthermore, make one seriously question the role of editors and press.

A determined editor with a sharp pencil might have turned this overlong essay into a good short article for a legal journal.

STEPHEN E. PATTERSON
University of New Brunswick

ADELE HAST. *Loyalism in Revolutionary Virginia: The Norfolk Area and the Eastern Shore*. (Studies in American History and Culture, number 34.) Ann Arbor: UMI Research Press. 1982. Pp. 227.

A contagion of studies concerning the activities of the Loyalists in the American Revolution has occurred in the last twenty years. Every major Tory activist, and some who were not so consequential, has attracted a biographer. Books and articles chronicling loyalism in every state have confirmed or supplanted similar studies made half a century or so earlier. Scholars have explored Tory military activities and Tory ideology, Tory pension claims, and the lives of Tories in exile.

What has not been explored adequately? Surprisingly few case studies of loyalism in a local setting have seen the light of day, and Adele Hast has set out to correct that oversight. The result is an attractive and useful monograph that explores the behavior and experiences of Loyalists in two portions of Virginia. Hast looks in on Norfolk borough, the commercial center of the colony, and on

Accomack and Northampton counties on the Eastern Shore, an insular region inhabited primarily by small landowners and tenant farmers.

Hast is strongest on the Norfolk area. A mercantile and shipbuilding center of six thousand inhabitants in 1775, Norfolk's population was divided between native-born (and nativist) Americans and un-Americanized Scottish merchants. Even before the Anglo-American imbroglio tensions sometimes surfaced between Scots and Virginians; the Revolutionary struggle over implementing an American trade embargo against the parent state only exacerbated these divisions. The Scottish merchants of course resisted the various bans on trade that took effect in 1774 and 1775, and their differences with the popular party over this issue deteriorated into violent clashes in the early days of the war. Hast convincingly argues that ideological persuasions were unimportant in the choice between loyalty and resistance. Both sides, she demonstrates, shared the views of Whig ideology. In part she explains the actions of the Norfolk Loyalists by pointing to their economic interests. But she goes beyond that and in her most significant contribution Hast shows that kinship, familial ties, also was a factor in determining one's loyalty.

The Eastern Shore was a very different matter, however; not only was it economically and demographically unlike the Norfolk area, but also the Tories of the region subsequently behaved differently than their more urban compatriots to the south. During the war, for instance, they engaged in fewer overtly hostile activities against the popular cause. Hast stumbles a bit here, never seeming to be quite certain what motivated these people to remain loyal to their king. In the end she suggests rather lamely that the small farmers in these isolated counties simply concluded that they stood to gain nothing by being independent of Great Britain.

Specialists will discover much helpful data in this study. Hast's analytical comparison of the Tories who chose exile with those who elected to remain in Virginia is useful, as is her demographic profile of those who proclaimed their loyalty. The mild treatment of the dissidents by Virginia authorities—in surprising contrast to the fate suffered by Loyalists elsewhere, especially in other Southern states—is detailed. She also accounts for the occasional martial endeavors of the region's Tories.

Hast has produced a well-researched and lucidly written account of Toryism in a small sector of Virginia. As additional case studies of Loyalism are forthcoming, we will be better able to assess the uniqueness of the motives and experiences of these Virginia Loyalists.

JOHN E. FERLING
West Georgia College

OSCAR HANDLIN and LILIAN HANDLIN. *A Restless People: Americans in Rebellion, 1770–1787*. Garden City, N.Y.: Anchor Press. 1982. Pp. 274. \$14.95.

The last book to attempt a broad survey of Revolutionary society appeared seventeen years ago. Since then, historians have greatly improved our knowledge and methodological techniques, and we have also become aware of the need to recapture a mass audience by combining meticulous research with an interesting prose style. Oscar Handlin and Lilian Handlin have succeeded in the latter at the expense of the former, and one wonders gloomily whether the two are reconcilable.

The Handlins attain an attractive prose style by several methods. They generalize freely, avoid qualifiers and numbers, insert effective quotations, illustrate their points by biographical sketches, and present a vivid, consistent interpretive line. Each has its hazards. We read that "Men" attended to their locks or wigs daily (p. 58) whereas only a few did so. Most of the urban poor "wallowed in filth and their wives waded to the knees while carrying a loaf of bread to bake" (p. 45). Surely not all frontiersmen were "fugitives from civilization—vile rascals, drunkards, and wretches" (p. 133). The gambling of William Byrd III was extraordinary, not representative; it fits the thesis but insults Virginians. So also Deborah Sampson does not typify the Revolutionary female. The absence of statistics and footnotes will please the average reader but dismay the specialists seeking evidence.

The book's title states the interpretation: Americans were restless, or, in other favorite words, unsettled, unstable, insecure. The thesis pervades every chapter, even that on the family, in which we read that "few individuals could relax, feel secure in a community" (p. 93), and separation from a family was "the norm" (p. 86). People were "constantly off balance" (p. 78). After the war "society, always unstable, now quivered incessantly to the shock of recurrent unexpected changes" (p. 154). American culture was at its best when it responded to people's "restless needs" (p. 196). The book closes by stating that "a restless people found itself ever confronting new challenges and, generation after generation, recklessly abandoned inherited assumptions to explore new ways of life" (p. 240).

But are any of these statements true? Or, if valid for some people, of how many? Since many—perhaps most—Americans were not unsettled, and since Western Europeans were exceedingly mobile, the Handlins must demonstrate first a significant difference between the two, and second, what proportion of Americans fit their descriptions. The Handlins require us to take on faith what only new evidence can prove. The general reader will find the

book delightful. The scholar, however, may become restive and unsettled.

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JOHN QUINCY ADAMS. *Diary of John Quincy Adams*. Volume 1, November 1779–March 1786; volume 2, March 1786–December 1788; Index. Edited by ROBERT J. TAYLOR et al. (Adams Papers, series 1, Diaries, Numbers 1 and 2.) Cambridge: Belknap Press of Harvard University Press. 1981. Pp. lxii, 415; xiv, 521. \$60.00 the set.

"Went in the forenoon, and pick'd blackberries with the young Ladies. Lucy Cranch tells me I have no Complaisance in me, and I suspected as much before . . ." (August 4, 1786, *Diary of John Quincy Adams*, vol. 2, p. 74).

Lucy Cranch may have misjudged her nineteen-year-old cousin. True, the serious, self-absorbed, rather humorless young man, so critical of others, hardly seems ingratiating. His diary, however, reveals other aspects of his character that prove very touching: vulnerability, sensitivity, affection for his family and friends. Even his pervasive condescension toward women appears in large measure a defense mechanism against his romantic tendencies. If he bends before duty it is not without a battle. His struggles are all too human—to persevere in his law studies he must overcome terrible anxiety not only about the limited opportunities for young lawyers but also about his own inadequacies. At the completion of his first year in the law office he publicly proclaims his willingness to accept a lesser station in life than his ambition demands, and then, falling ill, abandons his diary and his studies.

For explaining the crisis that serves as culmination to these volumes, the diary alone is inadequate. Begin instead with the several dozen letters between John Quincy and his parents in the four volumes of the *Adams Family Correspondence* published to date. John and Abigail Adams, however loving, laid daunting burdens indeed on their son; although he seldom writes directly about them, their presence (even when they are several thousand miles away) permeates the first two volumes of the *Diary of John Quincy Adams*. To read these nearly nine hundred pages is not easy, for although the writing is clear and sometimes eloquent there is much repetition and triviality. There are, however, many rewards for the perseverant reader, who will meet a young man of exceptional intelligence, idealism, and candor, somewhat stuffy and conventional perhaps, but engagingly human. The historian also will profit. In addition to providing insights into the character

formation of the future president, these volumes reveal much about his beliefs, attitudes, and values. Many more volumes of his diary are to follow (the abridged edition of a century ago totaled a dozen volumes); these earliest parts, hitherto largely unpublished, are an indispensable background for those that will follow.

Their interest, furthermore, extends beyond the personal. John Quincy Adams began his earliest diary at age eleven aboard a French warship returning to Europe. For the next six years he recorded his impressions of Spain, France, the Netherlands, Germany, Russia, Sweden, and England. In 1785 the intermittent and somewhat superficial entries are replaced by a far richer and more complete record of Adam's activities, beginning with his last few months in France. He then returned to Massachusetts and in the spring of 1786 entered Harvard. His diary gives a full account of his student life, complete with sketches of all his classmates, descriptions of his studies and recreations, and criticisms of his teachers. Soon after graduation he entered a law office in Newburyport to prepare for a career as a lawyer. His diary records not only his crisis of confidence but also the activities and diversions of a Massachusetts town. His observations will interest historians of American society, culture, politics, and religion. None of the topics in these volumes is unique, but outside the papers of the Adams family it would be hard to find a diary of such range and variety.

Outside *The Adams Papers* too, one rarely encounters editorial work of such superb quality. The scholarship is imaginative, accurate, and unobtrusive, the illustrations fascinating, the typography handsome, the index a model of its kind. John Quincy Adams eventually will prove a worthy heir to a great political tradition; his editors already have proved themselves worthy heirs to a great scholarly tradition.

JONATHAN R. DULL
Papers of Benjamin Franklin

DANIEL N. HOFFMAN. *Governmental Secrecy and the Founding Fathers: A Study in Constitutional Controls*. (Contributions in Legal Studies, number 17.) Westport, Conn.: Greenwood Press. 1981. Pp. ix, 339.

Daniel N. Hoffman's study of secrecy in government during the Federalist era throws needed light on the question of executive privilege. While the author argues that government can hardly err on the side of assuring access to information, he nonetheless explains that the decision to release information depends as much upon institutional imperatives and partisan advantage as legal rule or constitution-

al principle. The relevant law has followed the dictates of rather mundane concerns, even as the framework for debate has been shaped by an enduring faith in the constraints of public accountability and the rule of law.

Hoffman has consulted all the extant records to review every instance of political debate about secrecy during the years from 1787 through 1801. He concludes that the doctrinal trend of the law was ever toward free access, albeit in a fitful rather than linear course of development. By 1801, several points had been accepted. Congress, through either house, had the authority to call for papers on discretion. When the issue in abeyance had to do with a possible declaration of war or impeachment, the president's admitted authority to censor the papers submitted in response to such a call gave way. Once Congress had the papers, whether under presidential strictures of confidentiality or not, either house could decide by majority vote to publish them. Finally, no person had ever been subjected to contempt or criminal proceedings because of having released information classified as secret.

The law and procedures concerning secrecy in government developed simultaneously with the rise of political parties in the United States. There was undoubtedly a direct relationship here, since the original theory of countervailing powers within government to guard against tyranny proved unworkable in practice. As Hoffman shows, however, the party system itself soon became part of the problem during the so-called Federalist "reign of terror." As a result, and to provide the needed corrective, broad agreement about the possible abuses of power led to consensus concerning the law and procedures on secrecy.

Hoffman's basic argument builds on this conclusion. He points out that information exchange within government and between government and society usually proceeds without serious problem unless the country is embroiled in an international crisis and this involvement spawns differences among political groups. Under such circumstances, as during the 1790s and 1860s, secrecy in government leads to legal and constitutional controversies. During the two periods mentioned, Hoffman contends that the American tradition, customs, attitudes, and beliefs exerted a shaping influence upon the resultant partisan struggles by providing a universe of discourse within which debate occurred. Thus the basic concepts of popular sovereignty and the rule of law led in the end to increased emphasis upon the release of information so that an informed public could judge properly among the available alternatives. But Hoffman does not argue that ideas check ideas. Rather, ideas espoused by active political groups provide the needed check. In a word, power checks power, and the participants learn to respect

the constraints because of their recognition of the consequences of doing otherwise.

Hoffman's style of interweaving analysis and narrative works well to lead the reader through a complicated series of events while maintaining the focus upon information exchange and secrecy in government. Most readers will find much to ponder as they seek to respond to the calls today for official secrets legislation and enlarged presidential discretion. If an understanding of historical origins can enhance public debate, then Hoffman's book deserves reading.

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RONALD E. SEAVOY. *The Origins of the American Business Corporation, 1784-1855: Broadening the Concept of Public Service during Industrialization*. (Contributions in Legal Studies, number 19.) Westport, Conn.: Greenwood Press. 1982. Pp. xii, 314. \$29.95.

To many liberal reformers of the later nineteenth century the large corporation appeared to be the major instrument by which the wealth of the nation was being inexorably channeled into the hands of wealthy entrepreneurs at the expense of the people at large. But Ronald E. Seavoy, in this excellent study, contends that during the first half of the century the corporation performed exactly the opposite function: it mobilized capital and entrepreneurial skills for the development of public service enterprises whose growth would have been much slower and more difficult without the corporate form of business organization.

At the outset of his study, Seavoy shows that in New York the corporate form, reflecting the function of public service, was at first granted only to religious and educational institutions. But as the need for transportation improvement became very apparent, the corporate form was extended to turnpike companies, beginning in 1797, and thereafter to canal and railroad enterprises and banks. But since corporations entailed limited liability and other privileges not granted to independent businessmen, each incorporation required a special act of the legislature. This projected corporations into politics. In order to end this unhealthy development and equalize the access to corporate privileges, general incorporation laws giving uniform provisions for entry into the various areas of incorporated business were passed.

Yet these laws did not bring about a divorce of business and politics in all areas—particularly in banking. Perhaps the most original and illuminating part of Seavoy's book is his description of the intricate maneuvering of bankers and politicians under the Albany Regency of 1819 to 1838. Since

banks controlled public credit, a degree of stability was imposed upon them by the Safety Fund Statute of 1829. When this proved to be inadequate and only strengthened the union of banks and politics, the Free Banking Act of 1838 threw banking open to a much wider segment of the business world and became a model for banking legislation extending down to our own day. Henceforth, in New York, "General incorporation statutes transformed the corporation from an instrument of business privilege into an instrument of democratic enterprise" (p. 265).

Written from a wide range of primary and secondary materials—particularly legislative statutes and commission reports—Seavoy's book may serve as a model for tracing the history of incorporation in other states. But here, particularly in the South, hostility toward incorporation was much more pronounced than in New York and lingered much longer. And probably few observers, then or now, would agree without qualification to Seavoy's contention that general incorporation laws transformed corporations "from an instrument of business privilege into an instrument of democratic enterprise."

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HARRY L. WATSON. *Jacksonian Politics and Community Conflict: The Emergence of the Second American Party System in Cumberland County, North Carolina*. Baton Rouge: Louisiana State University Press. 1981. Pp. xii, 354.

In 1961 Lee Benson in *The Concept of Jacksonian Democracy* pioneered a new era in the historical analysis of political parties and electorates. He also drew attention to, among other things, the impact of the transportation revolution on social and political movements in antebellum America. Harry L. Watson's intensive study of a diverse Southern county builds pragmatically on the work of a variety of interpreters of Jacksonian parties and extends Benson's emphasis on the transportation revolution. Watson says that the latter was the principal agent in creating "rival communities" (p. 322) that eventually formed the constituencies of the Democratic and Whig parties in the 1830s and 1840s. In finding that communities divided along a town-commercial-cosmopolitan-Whig versus country-local-Democratic axis, Watson does not differ much (as he acknowledges) from the main conclusions of Sellers, Alexander *et al.*, Sharp, and Thornton (and, he might have added, F. J. Turner). But Watson's is the most detailed study yet of the emergence of party institutions in a Southern locality, and it is especially welcome since the South has been something of a

puzzle in that its party divisions were much less influenced by the religious and ethnic divisions so prominent in many Northern communities.

Watson asks many excellent questions about the nature of party constituencies and their mobilization, but the real strength of this work is the care with which it describes prepartisan political culture and the gradual transition to party organizations in the 1830s. Watson does not, as so many have, prematurely rush the emergence of parties and does not impose a mechanistic concept of "party" where it does not belong. Instead, he appreciates the persistence of traditional modes and the absence of party institutions and stable coalitions into the 1830s.

There are several flaws in this otherwise solid and useful monograph. At times Watson makes too much of data that does not justify the effort or the claims attending it: for example, his conclusions based on correlation coefficients derived from only ten, eleven, and thirteen precincts (pp. 209–13) and 18 pages of guess-filled exegesis of the social attributes of seventy-five political activists of 1828 (pp. 127–46). Elsewhere a dash of skepticism toward evidence would have been in order, especially in the case of an alleged "farmer's" letter (pp. 283–85) that has the look of a classic put-on of the period. Given the book's length and scope, author and editor might have tightened and shortened its text.

Watson's overall thesis is reasonable, though not convincing in all its parts. In his conclusion he himself points to one possible way that his analysis might have been strengthened. In a too-brief discussion of North Carolina as a whole, Watson observes that though the state divided rather evenly between Whigs and Democrats, the state aggregate hides a condition of polarization among most counties, which were lopsidedly Whig or Democratic. Watson suspects that this "problem of unanimous areas" can be attributed to the persisting power of "rural oligarchies" and eighteenth-century deferential relationships (p. 311), and refers in his notes to Whitman Ridgway's book on Maryland elites. The reference is quite appropriate, and more might have been done with the kind of comparative approach used by Ridgway and others, both within North Carolina and within Cumberland. Indeed, the recent work of Paul Bourke and Donald DeBats also fits nicely with Watson's emphasis at points on neighborhood voting blocs, but that may have reached Watson too late. Nevertheless, a slightly more comparative focus, with less detail about Cumberland, might have led to modification of the emphasis Watson places on the transportation revolution.

Watson's evaluation of the party system, finally, is somewhat ambivalent. His tone throughout is mostly one of appreciation for the pragmatism of Ameri-

can parties, and he also takes some trouble to defend the social conscience of Democrats and the genuineness of their critique of commercialism on behalf of the have-nots. Then, in his final paragraph, he delivers a succinct analysis of the essentially conservative social function of the party system, the tone of which differs strikingly from that of the rest of the book. Ambivalence may very well characterize his basic posture toward political parties, as it has and does that of so many other Americans.

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MERRILL D. PETERSON. *Olive Branch and Sword: The Compromise of 1833*. (Walter Lynwood Fleming Lectures in Southern History.) Baton Rouge: Louisiana State University Press. 1982. Pp. 132. \$15.00.

This contribution to the Walter Lynwood Fleming lecture series focuses on the nullification settlement. Or rather, Merrill D. Peterson focuses on leaders whom he believes created the compromise. "The history of the Compromise of 1833," Peterson argues, "is largely the history of Clay and Calhoun, Jackson and Webster" (p. 126). Peterson sees these leading statesmen as personifications of larger trends and prime influencers of major legislation. Peterson's three chapters follow the dominant "few leaders" from nullification through compromise to the legacy of settlement.

The "Great Man" approach both underlies Peterson's successes and limits his achievement. The author subtly distinguishes between epoch-makers. For example, he nicely contrasts the different world views Clay and Calhoun brought to their anti-Jackson union. Clay saw King Andrew's despotism as the problem and congressional economic nationalism as desirable. Calhoun saw executive usurpation as rooted in undesirable congressional nationalism. The Whig party was thus "badly divided" from the moment of "its schizophrenic origins" (p. 99). Such analysis whets appetites for Peterson's projected volume on Clay, Webster, and Calhoun, of which *Olive Branch and Sword* is an early offshoot.

One hopes that the larger volume will come to terms better with limitations of the "Great Man" approach than does this initial foray. Because Peterson focuses narrowly on the few who personify and influence, his wider history of national crisis adds little information not standard in the nullification literature. Nor does the author leave room for much consideration of those social and economic forces that influencers supposedly personified.

One has, as a result, a sense of superficiality, of puppets dancing with no strings evident. But the more disconcerting effect is a sense of distortion;

allegations of influence remain undemonstrated. Henry Clay, for example, emerges as architect of compromise. Clay is portrayed concocting lower tariffs, urging protectionist supporters to accept unprotectionist rates, speaking movingly in behalf of the Union. But even Peterson's overly fragmentary voting analysis indicates Clay won over few of his own partisans. New England, center of Whig support, opposed Clay's bill. The South, Jackson's stronghold, massed behind compromise.

The compromise of 1833, allegedly "Clay's," thus bears suspicious resemblance to the Compromise of 1850, once also alleged to be "Clay's." Clay's oratory aside, the later great compromise is seen now more correctly as a Democratic settlement passed by Stephen A. Douglas supporters. Peterson's earlier Clay may have been equally powerless without some yet unexplored source of help.

If the essay does not prove Clay to be prime mover in passage of the compromise, neither does it substantiate that John C. Calhoun personified the legacy of settlement. Calhoun liked to believe that nullification triumphed in 1833; he was still trying to effect nullification in 1850. Peterson makes the "Great Man" symbolize his area; South Carolina too believed nullification had worked. That conclusion misses Calhoun's isolation. Most South Carolina fire-eaters were so unnerved by their exposed position in 1832-33 that they gave up nullification. Calhoun spoke for no other South Carolinian in his culminating *Discourse and Disquisition*, which urged that nullification be written into the Constitution. In the end, Peterson's "Great Man" history steers toward the sharpest rock threatening this genre: by minimizing the influenced, the historian may lose sight of when his influencer loses all influence.

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GAY WILSON ALLEN. *Waldo Emerson: A Biography*. New York: Viking Press. 1981. Pp. xxiv, 751. \$25.00.

A biographer of Ralph Waldo Emerson faces a surprisingly challenging task. Emerson was a supremely articulate man, recording his thoughts in letters, journals, lectures, essays, and poems, yet he remains stubbornly elusive. Why as a young man did he spend the best part of a decade in a ritual encounter with the ministry, hesitantly entering then quitting in protest a profession he sensed from the start was not for him? Why did an intellectual nearing forty form some of his closest ties with Henry Thoreau and others a generation younger than he instead of with his peers? Why, at sixty, did Emerson find he had no new thoughts? Gay Wilson Allen's *Waldo Emerson: A Biography* aims to make Emerson more accessible than have previous biogra-

phies (most recently Ralph Rusk's in 1949) and to move him out of such genteel company as triple-named Henry Wadsworth Longfellow and into the camp of plain Walt Whitman. A thoroughly researched and well-written narrative, the book engages the reader's imagination with significant questions, but stops short of supplying the answers necessary to the intimacy Allen intends.

More deliberately than past biographers, Allen tackles the relation of the inner and outer man. How is it that a thinker of great erudition given to mystical moments looked to one observer in 1863, when acting as a visitor to the military academy at West Point, like a shrewd Yankee farmer checking the stock? Allen juxtaposes events and ideas throughout the study, too often, however, without explaining their mutual impact. It is also surprising that he insists so narrowly on Emerson's debt to Neoplatonic thought; despite Emerson's professed reservations on the utility of books, he was a reader of staggering breadth, eclectic throughout. Nor does the search for literary influences tell us why this Yankee, with his crops and kin, conscience and reforms, found idealism to suit his wants or why his audiences, less esoteric surely than he, eagerly listened.

Allen does set out to show Emerson in his historical context and describes his early years—also the first decades of the nineteenth century—especially well. The money-getting expedients of the minister's widow, the four brothers' Harvard experience, and Waldo's dogged efforts to communicate with Ellen, his first wife, after her death give a fresh angle on early republican society. In contrast, uncharitable judgments of Emerson's friends and relations mar Allen's study of later decades. Bronson Alcott is an "eccentric schoolteacher," and some of Emerson's visitors are cast as "freaks of various kinds" (pp. 254, 252). More seriously, Lidian Emerson, his second wife, appears mainly as a hypochondriac, which she apparently was, but available manuscripts, some recently edited by Delores Carpenter, also show Lidian a supportive wife and intelligent woman in her own right. The magnification of Emerson to heroic proportions has been a chronic problem of Emerson scholarship. Allen's handling of the man himself is judicious, but his tendency to dismiss Emerson's peers leaves Emerson standing a head above the rest, more like Ralph Waldo, after all, than Waldo pure and simple.

As a traditional literary biography, *Waldo Emerson* can justly be praised. But precisely because the book is a narrative exposition of Emerson's life and works, Allen's intent to explore the connections between ideas, experience, and historical setting has been satisfied only in part.

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STEPHEN FENDER. *Plotting the Golden West: American Literature and the Rhetoric of the California Trail*. New York: Cambridge University Press. 1981. Pp. ix, 240. \$24.95.

"Why should not we also enjoy an original relation to the universe?" is the fifth sentence of Emerson's *Nature*, published in 1836, and a question still worth asking. The innocence of "enjoy" is tarnished of late, but in the American context conceived by a sympathetic English scholar "original" goes by the good old ratio of California is to the United States as the United States is to the Old World. The title of Stephen Fender's book, which names much, omits two big points: what is meant here by American literature is centrally California literature; and the word "literature" is understood to mean writing inclusively, whether of Eastern men of letters (Irving, Hall, Flint), government-sponsored explorers (Frémont and his *Report*), the forty-niners (their diaries and journals: one chapter for men, one for women—women's diaries are better); journalists in both kinds; and Mark Twain. California literature means writing about California by outsiders, there being at the time hardly any others.

The consideration is that California was a new experience for all these people; that they wrote for an audience "back East"; that they employed literary conventions and stylistic instruments familiar to that audience and to themselves; that the formal conventions and stylistic instruments fit as well as might be expected, and seldom well enough; that things gave way and were adjusted. Romanticism yielded to factuality, felt as romantic enough, or anyway outrageous, it not being often clear, then or now, if California offends chiefly in its climate (too good), its landscape (too big), or its inhabitants. "The West [California, mainly San Francisco], as it appeared to these professional journalists, was as unruly and out of scale as that perceived by the forty-niners crossing the country, and the journalists were as articulate as were the forty-niners about their situation 'on the borders of civilisation'" (pp. 106–07). That situation was to become reflexive and permanent. San Francisco "is a city that talks about itself a great deal," (p. 108) especially the daily press. But "what needs to be insisted on, to return to critical issues, is how much western [again, Californian is meant] writing gains by the tension between raw and cooked in what is, after all, a literature mediating between civilization and barbarism" (p. 166).

The merit of Stephen Fender's fine book is perhaps most in its insistence that civilization is preferable, and in its further recognition that civilization is of various shapes and colors, some at first difficult to make out, depending on the degree of newness they present to the perceiving eye. *Plotting the Golden West* is beautifully organized and beauti-

fully written, and contains some wondrous illustrations, notes, an index, "An essay on further reading," all the appurtenances of careful and gracious scholarship. Fender's account of 1849 diaries and journals is based on first-hand examination in the Bancroft and Huntington libraries, precise historical scholarship thus linking us all the way from our old home to the last landscape.

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LILLIAN SCHLISSEL. *Women's Diaries of the Westward Journey*. (Studies in the Life of Women.) New York: Schocken Books. 1982. Pp. viii, 262. \$16.95.

This book is the first study of the Overland Trail to be based exclusively on women's diaries and reminiscences (103 in all, with four complete diaries included). Lillian Schlissel's purpose was to view the Overland Trail through women's eyes, and she has succeeded in her aim.

Schlissel shares some perspectives with other recently published studies such as John Unruh's *The Plains Across* and John Faragher's *Women and Men on the Overland Trail*. The major approach that Schlissel shares with Unruh is the division of the trail experience into earlier (harder) and later (easier) periods, plus a clear delineation of the cholera years. With Faragher, Schlissel stresses the importance of viewing westward migration not as an individual but as a family effort. Further, Schlissel stresses how the patriarchal family structure served to expand women's work activities on the trail, without expanding their decision-making power. This way of looking at women's situation has been current for some time, but Schlissel's detailed presentation of diary materials makes the case more convincing than before.

The picture of the westward journey that emerges from Schlissel's study differs in important respects from the received version. Her story is definitely antiheroic. Most women were reluctant pioneers who moved west because they put primary value on holding their families together. Their story of the westward journey is one of privation and loss. In one of her most striking observations, Schlissel points to the way in which women never lost their personal awareness of death. They counted graves. Schlissel says that women were "the actuaries of the road" (p. 15), never losing precise count, while men tended to aggregate (and thereby distance) the figures.

Women did everything they could to impose order and pattern on the endless Overland Trail journey, which often took two to four months longer than the original estimate. Schlissel's discus-

sion of women's work patterns is especially valuable in this context.

Schlissel places heavy emphasis on two central aspects of women's experience, pregnancy and child-rearing. Twenty-two percent of the women in her sample were pregnant. Another 50 percent already had children with whom they traveled west. (Most of the other "women" in her sample were under fifteen years of age.) Doubtless, as Schlissel claims, the reproductive cycle was central to the lives of most women. Yet the diaries are silent. Children are rarely mentioned, though their care must have been constant. Schlissel conjectures that child-rearing information was relegated to the female oral tradition, and the same may be true about pregnancy.

But the plain fact is that silences are a problem that not even the most meticulous diary-reader can overcome. Childbearing and child-rearing probably were central to the experience of women, yet these are the two topics on which the diaries have the least to say. There are not yet enough general studies of childbearing and child-rearing in the nineteenth century to fill in the gaps.

Schlissel's effort to look at the Overland Trail experience through women's eyes is a considerable contribution. She shows quite clearly that there *was* a female perspective, more reluctant, more fearful, more invested with dailiness, than the male accounts that we have accepted as "the whole story." Yet even an account as meticulously drawn from diaries as hers cannot stand alone. Perhaps Schlissel's book can best be viewed as a pioneer to be joined later by a number of other studies that will present the world view of nineteenth-century women in its full depth.

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DWIGHT G. ANDERSON. *Abraham Lincoln: The Quest for Immortality*. New York: Alfred A. Knopf. 1982. Pp. viii, 271. \$16.95.

The Abraham Lincoln described by political scientist Dwight G. Anderson was driven by a profound, obsessional anxiety about death and by a monumental ambition into a *Quest for Immortality*. Only immortality, the living on after death in the memory of future generations, would satisfy Lincoln's ambition, which, Anderson writes, "preceded his principles . . . and in large measure determined what his principles would be" (p. 96).

Lincoln, the reader is told, at first sought to follow the principles responsible, according to his reading of Parson Weems, for the fame of George Washing-

ton: honesty, patriotism, self-control. By the late 1840s and early 1850s, however, those principles had not brought Lincoln the recognition his ambition craved, his political career seemed finished, and he was despondent and disillusioned. But, spurred by ambition, a new Lincoln emerged in 1854 "with a revolutionary vengeance" (p. 7), a Lincoln who now identified himself with Jefferson and the Declaration of Independence, who linked his own personal resentments against George Washington and the Constitution to the injustice of Negro slavery. Henceforth, this new Lincoln followed the model warned against both in Washington's Farewell Address and in Lincoln's own January 27, 1838, Lyceum Address, the model of a "cunning, ambitious, unprincipled" person who would seek greatness "on the ruins of public liberty" (p. 6).

As president, Lincoln maneuvered the South, in Anderson's words, "into striking the first blow" (p. 166), and waged "fratricidal war in order to achieve his own 'elevation'" (p. 201). Through his wartime proclamations, messages, and addresses, Lincoln set forth a "political religion" in which divine authority was claimed for waging war, and war was pictured as the source of redemption and immortality through sacrifice, rededication, and symbolic rebirth. Lincoln won immortality and took Washington's place as the father of his country by becoming a "tyrant who . . . presided over the disintegration of the settlement of 1789" (p. 99).

Anderson describes briefly what he terms "Lincoln's Legacy" to the twentieth century. The description is not crystal clear to me, but Lincoln seems to be the father of the Imperial Presidency, of the waging of war by the United States for imperialistic purposes, of the "justification for killing peasants in Southeast Asia" (p. 228), and of various other developments of which Anderson disapproves—just as he disapproves of ideologies ("American liberalism," "constitutional liberalism") that in his view have concealed the ruinous effects of Lincoln's legacy.

The evidence for Anderson's description of Lincoln consists primarily of interpretations of Lincoln's own words. Readers who wish to evaluate the validity of Anderson's interpretations can quickly examine one brief, but highly important and representative, example by comparing six words from Lincoln's 1838 Lyceum Address, "and I believe it is true," printed in context on page 236, with Anderson's interpretation (p. 246, n. 19) that those six words constitute "Lincoln's explicit statement of identification with those who might become tyrant." In this example and throughout the book, Anderson's interpretations of Lincoln's words seem to me unconvincing. Although the book is in some respects interesting and thought-provoking, its de-

scriptions of Lincoln are not justified by the evidence cited.

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ALLAN G. BOGUE. *The Earnest Men: Republicans of the Civil War Senate*. Ithaca: Cornell University Press. 1981. Pp. 369. \$28.50.

Once upon a time, a political historian who ventured into quantification was apt to provoke the outrage of "humanist" colleagues. Nowadays, however, the huffiest criticism of any computer-aided monograph is apt to come from other quantifiers. The methodological admonitions of a few (J. Morgan Kousser, for example) have established them as arbiters of what is and is not good work, and those without considerable scholarly courage are advised to think twice before subjecting themselves to the subfield's often impossibly rigorous standards. Thirty years ago, to extrapolate David Donald's view, the historian of the Civil War era may have felt it incumbent to establish an appropriate relationship to Abraham Lincoln. These days, as statistical analyses of the period grow more frequent, there seems less a need of "getting right with Lincoln" than of "getting right with Kousser."

For fifteen years Allan G. Bogue explored the quantitative frontiers of American political history and has had his run-ins with the methodological hostiles. But his adventures have paid off handsomely. *The Earnest Men* supplements, in an enormously authoritative manner, our knowledge of mid-nineteenth-century Republicanism and is clearly one of the best American legislative studies, if not the best, extant. Bogue writes with a wary eye to statistical sophists but also with compassion for the vast majority of middle-period Americanists—that is, those far more comfortable with the *DAB* and the *OR* than *SPSS*. And, unlike some quantifiers, he has not neglected his homework in the letter collections, memoirs, newspapers, and secondary works. He also believes that good, even elegant writing is a legitimate inheritance from the days when history was mainly a literary art. His book combines the best of the old and new traditions of the profession—the very model, one might be forgiven for saying, of the modern major monograph.

The author's statistics are offered modestly and only when demanded by the job at hand. For the Civil War Senate that job is mainly twofold: (1) empirically dividing Republicans into radicals and moderates, and (2) obtaining objective measures of disagreement between factions. No single-method evangelist, Bogue brings a variety of techniques into play, an eclecticism lending more than ordinary

credibility to his statistical findings. Part 1 of the book—"Men, Context, and Patterns"—disposes of these questions by way of laying an unimpeachable foundation for part 2: "The Substance of Disagreement."

The going gets a bit heavy as Bogue explicates the issues his mathematics have revealed as most dividing radicals and moderates. The last two chapters, however, neatly knit things back together, and in them the author offers his larger judgments. His criticisms of others' conclusions are usually not very specific and are thus a little hard to isolate. The most interesting of these, perhaps, are four thoughtfully revisionist propositions.

(1) Republican senatorial unity recently has been overemphasized. The difference between radicals and moderates was real, "tempered by a relatively small and closely interrelated set of attitudes, of which those concerned with constitutional interpretation and race were the most important." Radicals were more vindictive than moderates, less racist, stressed the exercise of federal powers against the moderates' defense of state rights, and proved less respectful of the presidency, the judiciary, procedural and administrative precedents in their own body, and the Constitution.

(2) The much-abused Beard-Beale thesis deserves a rehearing. Radical Republicanism may not have been the precise agent of Gilded Age capitalism, but "the attitude of various radicals upon the proper relationship between the states and the federal government was certainly such as to gratify the industrialist interested in untrammelled development of the nation's economic potential."

(3) "Some historians to the contrary, it is amazing how little floor time the senators gave to reconstruction even during 1864."

(4) The current argument that "radicals or no, the Republican party's solution to the dilemmas of reconstruction was essentially conservative" grossly oversimplifies. In the first place, such a position is ahistorical. And "to argue that a legislative solution was not radical because more radical solutions were rejected may be to ignore the moderate or conservative alternatives that were also rejected. The degree to which a specific law incorporated radical or moderate proposals is surely the best measure of whether it was a radical or conservative solution to the issue at hand."

Bogue briefly wonders if a truly profound causal pattern underlay radical-moderate divisions, something partially documented by constitutional and legal historians. "May I suggest that perhaps there was a broader continuum of attitude among national legislators during, and before, the Civil War," he writes, "of which the poles may be termed modernizing-instrumental on the one hand and traditional-formal on the other, and that it is this structure of

attitude that we detect—imperfectly because it was, after all, imperfect—when we make distinctions between radicals and nonradicals?" Such a formulation, he adds, even makes sense of varying degrees of racism.

Allan Bogue has given us much to ponder.

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JOSEPH G. DAWSON III. *Army Generals and Reconstruction: Louisiana, 1862–1877*. Baton Rouge: Louisiana State University Press. 1982. Pp. 294.

Some years ago James Sefton published a general account of the U.S. Army's role in the Reconstruction process, remarking that enough material existed to justify short monographs on each Southern state. Now Joseph G. Dawson III has written such a study of Louisiana, where the army's role was particularly crucial for fifteen years. Based on extensive research from the military records in the National Archives, personal correspondence of army commanders with national political leaders, newspaper accounts, and published reports of congressional investigations, the book adds significant detail to our knowledge of Reconstruction in Louisiana but breaks little new interpretative ground.

Dawson begins with a brief discussion of the wartime regimes headed by Benjamin F. Butler and Nathaniel P. Banks. He does not always make effective use of the rich secondary literature on Civil War Louisiana; he confuses some political details and neglects the army's controversial policies toward freedmen's affairs. The primary focus of his research is the postwar period; indeed, the narrative grows stronger with each chapter. Biographical treatments are most effective for Philip H. Sheridan, Joseph A. Mower, and Lewis Merrill (committed Republicans), Winfield S. Hancock (a partisan Democrat), and the ambivalent professional soldier William H. Emory.

The most important issue with which the army had to deal was political violence. Dawson confirms Allen W. Trelease's view that, wherever federal troops were used, the armed bands of Democrats avoided violent confrontation. Commanders had so few men at their disposal, however, that they were unable to contain effectively the use of night-riding tactics, armed intimidation of prospective black voters, and assassination of Republican leaders. Anyone unconvinced by Joe Gray Taylor's account of the extraordinary use of violence by Louisiana Democrats after the Civil War will find fresh evidence here, persuasively documented and often

grippingly narrated. Dawson also shows how the political preferences of the generals shaped their use (or lack of use) of military power. He attributes the surprising Democratic victory in the presidential election of 1868 to the willingness of General Hancock to look the other way while the Knights of the White Camellia and other similar groups carried the day by gunpoint.

At the close Dawson characterizes the army's experience with Louisiana Reconstruction as an "ordeal." Some of the generals thought it was improper for the military to play so continuous a role in policing elections, especially during the last years of Reconstruction. Others, like Sheridan, Mower, and Merrill, thought it important for the army to preserve the essential functions of American government from the violent assaults of lawless Democrats (Sheridan called them "banditti"). They agonized because their few troops were unable to perform this task in the face of thousands of armed Democrats across the lines. President Ulysses S. Grant shared their concern, but in the end he gave in to the growing Northern demand for "peace" at any cost. This demand, ironically, was fed by the inaccurate, but widely held, belief that the army had an iron grip on a defenseless South.

PEYTON MCCRARY

University of South Alabama

JAMES E. KERR. *The Insular Cases: The Role of the Judiciary in American Expansionism*. (National University Publications; Multi-Disciplinary Studies in the Law.) Port Washington, N.Y.: Kennikat Press. 1982. Pp. vii, 131. \$16.50.

James E. Kerr's book reviews the celebrated "Insular cases" to show how judicial review—used whenever the Supreme Court looks at governmental action in light of the Constitution—brought about the doctrine of incorporated territory. As finally understood, this doctrine means that American territory is called incorporated when the entire Constitution applies to it; otherwise, such territory is called unincorporated.

Since 1867 questions concerning noncontiguous territory have arisen frequently. Current efforts to achieve statehood or independence for Puerto Rico (considered at some length by Kerr) illustrate the doctrine of unincorporation well. Puerto Ricans are American nationals whose governmental system reflects civil law tradition; hence, Puerto Rico is unincorporated.

Kerr's study shows that legal and political questions emerging in the territories have been pressing since the "splendid little war" of 1898. He includes brief biographies of the justices serving on the Court from that date, showing that the justices' earlier

experiences—of Brown in the business world and of White on the high court of Louisiana, for example—were significant in developing the doctrine of incorporation.

Short biographies of lawyers who argued these insular cases are also included. Kerr's study is to be commended for its careful use of their briefs. Deductions therefrom, and evaluations of justices and attorneys alike, seem fair and judicious. Kerr's study penetrates into a complex jungle of decision, opinion, dissent, and concurrence.

His work suffers, however, from an inability to exploit preliminary studies that might have been helpful. Use of doctoral dissertations could have strengthened Kerr's argument. Claims made for Alfred Thayer Mahan as an expansionist might also have been reviewed.

Kerr has utilized Finley Peter Dunne's Mr. Dooley, whose epigraphs stand at the head of each chapter. Mr. Dooley was no kindly humorist, but a savage satirist, as he is quoted in this study. Mark Twain might have used him for a model; Will Rogers, never.

Kerr's book could have used more vigilant editing: "waivered" for "wavered" (p. 18) is comic; "elucidate on" (p. 697) is bad usage of a transitive verb. Kerr escapes most jargon traps but is sometimes unduly influenced by the turgid prose of his sources.

On the whole, Kerr's book is a valuable addition to an understanding of the doctrine of incorporation; the subtitle is less well explicated and might have been omitted.

MARIE C. KLINKHAMER

Norfolk State University

WALTER J. FRASER, JR. and WINFRED B. MOORE, JR., editors. *From the Old South to the New: Essays on the Transitional South*. (Contributions in American History, number 93.) Westport, Conn.: Greenwood Press. 1981. Pp. xiii, 286. \$35.00.

In the last few years an emphasis on continuity has threatened to regain ascendancy over the theme of change in the interpretation of Southern history. I say "regain" because continuity had always been the preferred and predominant emphasis among writers of Southern history, whether of right, left, or center, down to the middle of the present century. Then for twenty years or so the emphasis swung to change and discontinuity. This deviation is attributed in the volume under review and elsewhere to the influence of a few historians, among them (with undue emphasis it would seem) the present writer. That miscreant cheerfully accepts his legitimate share of responsibility but suggests that perhaps impersonal forces were of greater influence. It

happens that the years of historiographical stress on change and discontinuity coincided with the most disruptive period of change and discontinuity the South had undergone for nearly a century. Whirl was king and change the rule: "Hurricane Charlie," it might be called. In the teeth of its gales little could be heard about continuity. Now that the winds have subsided, strains of the old chorus are heard once more.

The contributors to this symposium, held at the Citadel in Charleston, are not among the extreme exponents of the continuity reaction. Theories of the latter are summarized, but the proponents are not represented. While the major question before the participants is "whether the shift from the Old South to the New was characterized more by change or continuity" (p. xi), most of them sensibly concede the presence of both. Some do not address the question directly. Among those who do a few still speak up for the theme of discontinuity, but most of them lean the other way. Two essays achieve a rarified degree of ambiguity.

David Herbert Donald concedes that there was indeed a "sharp deterioration of race relations that occurred during the final decade of the nineteenth century" (p. 3), but he rejects all previous explanations in favor of his psychological theory attributing the rupture to the "age-cohort" (p. 7) of the Confederate veterans. The cohort theory is not easily susceptible to proof or disproof. Since it holds that "segregation and disfranchisement should be viewed as the final public acts, the last bequests, of the Southern Civil War generation" (p. 7), and links these changes with "reverence for the Lost Cause" (pp. 17-18), the argument suggests a circular form on the issue of continuity. Continuity appears to result in discontinuity. In his essay on "The Ambivalence of Change," William L. Barney plumps for continuity but supports it with evidence that the commercial Whigs of the antebellum South anticipated the program of the postbellum order and that "this Whiggish call for a New South was intended to vindicate the old South" (p. 36). Again the logic takes a circular shape. (Incidentally, Barney shows a stunning drop in per capita wealth of adult white males in his sample Alabama county from \$19,068 to \$3,525 between 1860 and 1870—which does not sound much like continuity.)

Three excellent papers on urban history by Michael P. Johnson, John P. Radford, and Don H. Doyle all support the thesis of continuity in social structure, elites, and race relations. But all three essays are on Charleston, the very citadel of resistance to change and scarcely typical of more than a few old seaport cities. In fact Doyle is the author of other studies of cities in the interior South that he pictures as bristling with change and full of a bustling new businessman of the Grady type push-

ing aside and replacing the old order and its elites.

On the emergence of a new entrepreneurial elite to replace the old planters, David Carlton presents the clearest evidence in his study of early industrialization in South Carolina. His findings challenge advocates of "continuing planter hegemony" in the industrialization of the New South, a thesis that Carlton calls "a serious misinterpretation of postbellum Southern history" (p. 44). His detailed statistics show plainly that planters "played comparatively minor roles in the industrial movement," rarely served as mill presidents, and as board members were "significant in only a handful of cases" (p. 48). New men, townsmen, mainly small-town businessmen ran the industrialization show with vital Yankee help—and this in South Carolina, showcase of Old South continuity.

Additional support for the heresy of discontinuity comes from Ronald L. F. Davis on freedmen and planters after emancipation. In the Natchez district, at least, "planters were dragged kicking and screaming into the system [of sharecropping] because blacks refused to work in any other system of labor" (p. 158). Few of the antebellum planters who persisted as landholders "were planters in the antebellum sense of the term" (p. 159) as active resident managers with centralized control. The sharecropping and lien system was to prove an economic trap for black as well as white croppers, but the new system was indeed new and not a continuity of the older order.

A study of white crime by David J. Bodenhamer suggests that the important distinction was not between a premodern and a modern society, but between rural and urban areas. On the other hand, William F. Holmes finds a persistent sectional trait in the South's resort to collective violence in maintaining traditional values. Whether or not change is the rule in the realities of history, it proves the rule in the myths of history as seen in Stephen Davis's essay on the fictional images of Johnny Reb since Appomattox. Four distinct perceptions emerge at intervals in a century of Southern fiction, each pointing a different moral. By contrast, the film of *Gone With the Wind* is presented by Edward D. C. Campbell, Jr., as evidence of the durable persistence of myth prevailing over both fiction and history.

No latterday revival of continuity and consensus would be complete without a tribute to W. J. Cash. This is provided in a witty, spirited, if somewhat perverse defense by Bertram Wyatt-Brown. Oddly enough, the tribute turns into a lament comparing the "former popularity" with "the current ill favor of Cash's *Mind of the South*" (p. 195) and lamenting that its themes of "the continuity and solidarity of Southern life went begging" (p. 197) among historians. And this in a day when no proper continuitarian, whether Marxist, conservative, or liberal, neglects to acknowledge indebtedness to Cash!

Lawrence Goodwyn, probably feeling somewhat out of place here, begins an eloquent essay on "Hierarchy and Democracy" with words that promise ringing dissent: "The tragic discontinuities of the Southern heritage beckon to poets with vivid signals of paradox" (p. 227). Before long, however, he is talking about continuity himself—but of a different type. Goodwyn is perfectly aware of the "tragic discontinuities" that characterize Southern history and is quite disdainful of their denial. Here, however, he is referring to continuity in "the rigidity of the old hierarchical ways of thinking" (p. 236) and regretting that "the apologetic tradition of Southern historical writing remains intact, imprisoned in nostalgia and defensiveness" (p. 237). Again the argument is circular, even if it arrives at quite a different quarter of the circle.

In all fairness, none of these historians would seem to subscribe to linear continuity, the orthodox form of the doctrine. They are by and large unorthodox continuarians, members of one or another dissenting sect of circular continuity. Perhaps there is hope for them yet. The orthodox are another matter.

C. VANN WOODWARD
Yale University

NEWELL G. BRINGHURST. *Saints, Slaves, and Blacks: The Changing Place of Black People within Mormonism*. (Contributions to the Study of Religion, number 4.) Westport, Conn.: Greenwood Press. 1981. Pp. xix, 254.

On June 8, 1978, the First Presidency of the Church of Jesus Christ of Latter Day Saints announced that the Lord "by revelation" had confirmed that "all worthy male members of the church may be ordained to the priesthood without regard for race or color" (pp. 178, 235). That statement reversed a 130-year-old policy of excluding black men from the Mormon priesthood. Newell G. Bringhurst has carefully traced the origin and development of LDS racial ideas and their effect on the church's policy and practice from 1820 to 1980.

About three-quarters of the study concerns the period up to 1865, during which time three lines of racial thought and practice developed among the Latter Day Saints. The first was a cosmic racial theory expressed in Mormon scriptures, in which positive racial universalism was intertwined with the restrictive idea that dark skin was divine punishment for disobedience. Bringhurst observes that these notions were not very different from the views of other Americans, who often used religious language, including the idea of divine punishment, when speaking of African-descended people.

Mormonism's second racial theme was also consis-

tent with general opinion in the North. This was the view that expansionist proslavery doctrine and "immediate" abolitionism were twin extremes that would bring the nation to ruin. The LDS position, however, was complicated by the legalization of slavery in Utah.

The third race-related element was the practice of excluding black Mormons from the priesthood, which includes nearly all practicing LDS males over the age of twelve. Priestly ordination is a prerequisite for receiving certain ritual blessings in the temple. Women qualify for these blessings through the priestly status of their husbands. Exclusion from the priesthood was therefore a serious religious impairment. Bringhurst believes that the exclusion became fixed shortly after the assassination of the founder, Joseph Smith, in 1844.

In dealing with the period after 1865, the author focuses on the persistence and justification of "black priesthood denial." But he also writes about the church's reluctance to send missionaries to blacks overseas, about Mormon attitudes toward the civil rights movement, and about discrimination against black people in Utah. All of these elements come together in his description of events leading up to the 1978 announcement.

Bringhurst's documentation is broad and extensive. His sources include a number of items in the LDS church archives in Salt Lake City, opened only in the last decade. He also used a lot of newspaper material, both Mormon and non-Mormon. His writing, though sometimes repetitive, is generally clear and straightforward. The book is an excellent treatment of an important part of American religious life. Bringhurst succeeds in showing the Mormons as a microcosm of the American population. He also offers a great deal of bibliographical and statistical information that will assist scholarly efforts beyond the scope of this study.

LESTER B. SCHERER
Eastern Michigan University

DURWARD T. STOKES. *Company Shops: The Town Built by a Railroad*. Winston-Salem, N.C.: John F. Blair. 1981. Pp. 169. \$14.95.

In January 1856, the first train passed over the newly completed North Carolina Railroad—some 223 miles of track commencing at Goldsboro in the east, winding northwest through Raleigh and Greensboro, and then dropping southwest to Charlotte. Shortly before the line was completed, its directors began construction of a general headquarters and maintenance shops in Alamance County near the line's midpoint. The development of this tiny cluster of railroad facilities, initially called "Company Shops," down to its change of name to

Burlington in 1887, is the subject of this brief exercise in local history.

Durward T. Stokes is at his best in tracing the fortunes of the shops during the antebellum and war years. The key figure before the war was Charles F. Fisher, the line's enterprising president, who supervised construction of the large brick maintenance shops, housing for the company's officers and the white and slave laborers, a depot and commissary, and a hotel—all the while fighting off dividend-hungry stockholders. War brought inflation and manpower problems, general deterioration, and some military destruction, but the NCRR adjusted well to the vicissitudes of politics and economics in the immediate postwar period. By 1870, the directors had done much to rehabilitate their line and rolling stock—only to be forced into consolidation. Faced with the Richmond and Danville Railroad's threat of constructing a competing line from Greensboro to Charlotte, the NCRR's directors agreed in 1871 to lease their entire line to the Virginia-based company. This marked the beginning of the decline of Company Shops as a repair center. Many of the employees were transferred during the 1870s, and in 1886 the entire maintenance operation was moved to Virginia. Confronted with the empty shops, disgruntled residents opted for the name Burlington the following year.

Despite the decline of the shops, the town grew after its incorporation in 1866. Lacking the resources to fulfill Fisher's vision of a company town, the directors began to sell and lease lots for housing and commercial purposes. During the 1870s sales were brisk as artisans, professionals, and merchants set up shop. Separate schools and churches for blacks and whites soon followed. Here Stokes's narrative frequently lapses into a catalogue of who founded what business, church, or school at what location. And other than noting that Company Shops had 817 inhabitants in 1880, the author gives no indication of the town's size, racial composition, and growth rate. Most notably, he fails to use the manuscript census schedules for 1860, 1870, and 1880—a rich and vital source for community reconstruction. Finally, his discussion could have benefited from Charles L. Price's 1959 dissertation on North Carolina railroads during Reconstruction as well as several recent dissertations on state politics, and he would have found Allen W. Trelease a far more balanced secondary source on Klan activity in Alamance County than J. G. de Roulhac Hamilton.

Overall, Stokes's account is readable, studded with useful maps and photographs, and adequately documented with newspapers and NCRR records. One is left with the conviction, however, that a more thoroughgoing investigation along the lines established in several recent Southern community studies

would have revealed more about the postbellum years of this town begun, not built, by a railroad.

TERRY L. SEIP

University of Southern California

ERIC H. MONKKONEN. *Police in Urban America, 1860–1920*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1981. Pp. xv, 220. \$24.95.

Eric H. Monkkonen's book *Police in Urban America* represents a valuable contribution to the contemporary scholarship on the nature and development of law enforcement in America. The strength of the work lies in the interdisciplinary examination of the adoption of uniformed police agencies by municipal governments between 1850 and 1900 and the development of a crime control role for the police, which has culminated in the schism between the police and those policed. Monkkonen's thesis is based on two tenets. First, he argues that police emerged as social control agencies as societies became progressively more complex. Urban governments developed water, fire, and sanitation departments to deal with specific problems, and the police correspondingly emerged in reaction to the crime problem in the city. The development of communication systems resulted in the diffusion of the police idea throughout the country.

Monkkonen's second thesis deals with the adoption of the police and the transformation of the police role in American society. With the demise of watch and ward systems, which were originally based on civic responsibilities for the community control of crime and the indigent, the police developed a more adversary model of police work. The original focus of the police shifted from the control of the dangerous classes to a crime control or deterrence model in which all segments of society became subject to the police presence. The separation of the public and the police in American society stands in contrast to Robert Peel's historic principle that the police are the public and the public are the police.

Given these two premises, the question must be asked if this is a history of police in urban America between 1860 and 1920. It is not. The book is an excellent account of several elements of police history: the examination of the diffusion of uniformed police services, arrest trends, and the treatment of tramps and children. Certainly the history of the police and their development is much broader, including issues such as their relationship to political machines, the development of professionalism, recurring corruption, and abuses of power. Monkkonen's scholarship runs counter to the nar-

row research on individual cities that has characterized much inquiry into the historical development of the police. Perhaps at a later time his work will be expanded to include a more detailed examination of the development of municipal government as well as employment and urban and industrial growth to constitute the promised history of police in urban America between 1860 and 1920.

Monkkonen's careful style of research should be studied by students of criminology who may underestimate the rigorousness of historical methods. The literature reviewed to support the research is comprehensive and ranges through many disciplines. The discussion of social control, for example, provided this reviewer with information on an important topic and is an example of the breadth of Monkkonen's work. This work is a valuable contribution to the historical and criminological literature.

MICHAEL D. WIATROWSKI
Florida Atlantic University

WILLIAM M. DE MARCO. *Ethnics and Enclaves: Boston's Italian North End*. (Studies in American History and Culture, number 31.) Ann Arbor: UMI Research Press. 1981. Pp. xx, 156. \$29.95.

ROBERT F. HARNEY and J. VINCENZA SCARPACI, editors. *Little Italies in North America*. Toronto: Multicultural History Society of Ontario. 1981. Pp. 210.

A proliferation of books about America's ethnics continues. The first of these two volumes contains nine essays that concern Italians in their enclaves. The editors, Robert F. Harney and J. Vincenza Scarpaci, argue that America's "Little Italies" have been too little studied for their own sake, and too much as appendages or portals to the dominant culture.

These essays were originally prepared for a conference sponsored by the University of Toronto and the Multicultural History Society of Ontario. Hence there are discussions of "Little Italies" in Montreal and Toronto, in addition to treatment of those in various parts of the United States. These cities were chosen in order to contrast the Italian-Canadian with the Italian-American experience. The conveners of the conference sought essays that would demonstrate what sort of immigrants were attracted to these ghettos. This reviewer would have been interested also in the motivations of those who refused to settle within congested *ambienti*, or why those who did so eventually left for greener pastures.

One learns much from these essays about fraternal organizations, mutual aid societies, foreign language clubs, immigrant unions, and church groups, both Catholic and Protestant. Restaurants, ethnic

newspapers, banks, boarding houses, and other ancillary organizations also formed a significant part of the social structure of these immigrant communities. Although there has been considerable previous writing about all these entities, the volume provides an updated account, with all the strengths and weaknesses of individual essays in one book.

George Pozzetta's piece on New York's Mulberry Street enclave is especially rich; but he could take advantage of the large volume of available evidence. New York, of course, was America's main portal. In the interior cities the ethnic records are undeniably sparser. This factor, however, scarcely explains the omission from this book of any ethnic community west of the Mississippi River. Why are there no accounts here of San Francisco, Los Angeles, or Seattle? Perpetuation of a lamentable East Coast bias results, despite an ambitious title. The most western city discussed is St. Louis in an essay by Gary Mormino. To omit virtually all the cities in the American and Canadian West skews the evidence as much as does that heroicized mythology to which the editors justly object in their preface. They do provide good inset maps, but each essay has not been carefully screened for appropriateness. That by Antonio Pucci, on Canadian industrialization, seems outside the book's announced theme. Also the lack of an index diminishes the usefulness of the volume.

Both this book and De Marco's *Ethnics and Enclaves* remind one that immigration history (now usually called ethnic studies) continues to feature stereotyped approaches to new knowledge. The way in which the study of minorities in North America has been carried on calls to mind William Langer's 1957 presidential address to the American Historical Association. His castigation of historians in general as being frozen in the molds of the past is especially pertinent to this branch of our discipline. Current ethnic historiography remains clouded not only by geographical limitations but in the measuring of the economic and social roles of minority members. We no longer need to deny that all foreigners, including the Italians, encountered prejudice and adversity on landing at Ellis Island. But their acculturative experiences did vary from section to section. More importantly, most immigration specialists make little attempt to "get inside" the immigrant's experience. They seem unaware of unconscious factors that flavored the immigrant's daily life. His resistances, needs for acceptance, and the mechanisms employed for the defense of ego still lay unexamined. Yet, there is a large body of post-Freudian psychiatric and psychoanalytic data available that remains unused.

De Marco's book does, however, consider with the utmost care the exterior life of Boston's North End.

Despite urban renewal (some of it attractive), its cobbled passageways, nineteenth-century tenements, and its warehouses reflect the ongoing vibrancy of this ethnic community. This is not a chronological study but a consideration of Old World values and how they have been retained. Working within a smaller compass than *Little Italies in North America*, De Marco is able to achieve greater unity. His familiarity with Boston's North End stems from his doctoral dissertation and earlier M.A. thesis. The resulting book fits well with other volumes in this series.

There is an overlap between De Marco's efforts and the essays in the *Little Italies* book. Both treat roughly the same social institutions. Although the former volume contains essays on Oswego, Philadelphia, and Tampa, it fails to include Boston. Also, De Marco seems more concerned with the role of women, marriage patterns, and tenets of *la via vecchia* (old ways of facing up to the American environment). Finally, his writing is, for the most part, felicitous—although statistics form a prominent part of this research.

For many decades Italian immigrants neglected to fill the gaps in knowledge about how they had fared. All that has changed. In the 1970s, as the third generation was on its way to being alchemized and blended into the dominant society, born-again descendants of the original immigrants no longer wished to keep their origins hidden in a closet. These two scholarly books seem to communicate almost a wish that resembles a Jungian collective unconsciousness—to get in touch with an ancestral homeland transported to these shores so long ago.

ANDREW ROLLE
Occidental College

JANET FORSYTHE FISHBURN. *The Fatherhood of God and the Victorian Family: The Social Gospel in America*. Philadelphia: Fortress Press. 1981. Pp. xi, 208. \$19.95.

From an examination of the writings of Walter Rauschenbusch, Washington Gladden, Lyman Abbott, Josiah Strong, William Newton Clarke, and Francis G. Peabody, Janet Forsythe Fishburn concludes that the Social Gospel movement sought to preserve the Victorian family ideal and its differentiation of sex roles. Representing the Kingdom of God in microcosm, the family, for these men, "set the pattern for personal and social relations that would characterize a fully realized democracy" (p. 24) and held the key to America's manifest destiny.

Rauschenbusch is the central figure and his *A Theology for the Social Gospel* (1917) the culminating statement of the movement. The others provide "context, contrast, and local color" (p. ix). Fishburn

divides the movement into two periods. Before 1900, Abbott, Gladden, and Strong focused on nonwhite Americans, labor, and urbanization, while for Rauschenbusch, Clarke, and Peabody in the period 1890–1924 the predominant issues were war, socialism, and feminism. Fishburn draws many contrasts but finds throughout a near obsession with the centrality of the family for "civilization" and its normativeness for individual and social morality.

Preoccupation with the family ideal was rooted in a Victorian "social revolution" that left the "meaning of manhood . . . problematic" (p. 21). Victorian men needed a formula for seeing themselves as both manly and moral. They found one in faculty psychology, which stressed subordination of passion to reason and moral sense and the complementarity of the sexes, and in liberal Social Darwinism, which accented the family's role in social evolution. Their idealized middle-class family became the measure of individuals, groups, institutions, and ideologies and the source of all their proffered solutions to social problems.

The family imagery used in Social Gospel literature suggests an inquiry such as this, and Fishburn provides useful commentary. There are deficiencies, however. She presents neither her psychological framework nor sufficient evidence for inferences about her subjects' attitudes toward their fathers, anxieties about success, and fears of the sexuality of women and of upper- and lower-class men. Her treatment of the relationships between their lives and ideas is unsystematic. She chose these six men because biographies or autobiographies exist for them yet neglects information that does not fit her scheme.

Though not applied rigidly, Fishburn's two-phase grouping oversimplifies matters. Men in the first group *were* productive after 1900, and their thinking changed. To discuss Gladden's views on labor in the 1870s and 1880s and ignore his response to Progressive era developments is to draw a static portrait. Fishburn's approach on other social issues is choppy, partial, perhaps even capricious.

Finally, Fishburn's understanding of the Social Gospel as essentially a form of individual evangelism leads to devaluation of its practical side. She portrays these men as hesitant Progressives, more interested in persuading others to conform to roles prescribed by the family model than in improving conditions. From this rendition, one learns nothing, for example, of Gladden's role in Columbus municipal affairs and support of settlements, or of how experience modified his thinking. Fishburn's intriguing thesis needs to be balanced by recognition of the Social Gospel's significant contributions to social amelioration.

JACOB H. DORN
Wright State University

CARL T. JACKSON. *The Oriental Religions and American Thought: Nineteenth-Century Explorations*. (Contributions in American Studies, number 55.) Westport, Conn.: Greenwood Press. 1981. Pp. xii, 302. \$27.50.

With discovery and exploration as guiding metaphors, Carl T. Jackson presents the first systematic examination of how nineteenth-century American thinkers responded to Oriental religions. Jackson sees three stages in the assimilation of new intellectual systems—first, hostility and apathy; second, gradual acceptance of whatever agrees with the truth of one's own ideas; and third, open acceptance of part or all of the new system. He outlines the first, primarily eighteenth-century stage but concentrates on the second stage and the transition, at the end of the nineteenth century, to the third. Emerson, Thoreau, Alcott, and Parker are at the center of Jackson's analysis; he uses the sources well and shows clearly the agreements and differences among these key figures.

Readers already familiar with the Transcendentalists will be especially interested in the way Jackson places leading thinkers in context by comparing them with the Free Religious Association and the later Transcendentalists and contrasting them all to the dark presuppositions of the missionary view of Oriental pagans. Later chapters treat the vogue of Buddhism inspired by Arnold's "Light of Asia," the achievements and aberrations of Theosophy and New Thought, the development of American Oriental scholars, the lure of Japan, Lafcadio Hearn's remarkable embrace of the East, and the 1893 World's Parliament of Religions. Jackson observes that admirers of Oriental religions concentrated on Eastern philosophical ideals, while detractors highlighted popular religious practices and deviations from traditional teachings. And he charts Transcendentalist subcurrents in later developments.

Jackson writes well and makes excellent use of notebooks and journals as well as published texts to illustrate his points. The bibliography is extensive, and there are enough wonderful quotations in the book to leaven many a lecture. My sole criticism is that, while Jackson tells so well the story of American thinkers encountering Oriental religions, he takes few interpretive risks regarding why all of this was happening and what it meant for American religious thought more generally. Percival Lowell observed that "the far-East holds up the mirror to our own civilization,—a mirror that like all mirrors gives us back left for right" (p. 207). Jackson is very good describing the mirror and the thinkers holding it; he is less satisfactory on the images it reflects. Yet one person's book is not another's, and Jackson has fulfilled the historian's responsibility—he provides the wealth of data others can use even to criticize his own interpretations.

This book is a major accomplishment, deservedly the winner of the Ralph H. Gabriel Prize in American Studies for 1979. It should be on syllabi for undergraduate and graduate courses, the desks of teachers, and the shelves of the libraries that serve them. I look forward to publication of Jackson's current project on Oriental religions and American thought in the twentieth century.

JAMES G. MOSELEY
New College
University of South Florida

ALAN TRACHTENBERG. *The Incorporation of America: Culture and Society in the Gilded Age*. (American Century Series.) New York: Hill and Wang. 1982. Pp. vii, 260. \$6.95.

Culture in the Gilded Age often has seemed to historians a fragile affair, defensive, uncreative, confused. Contemporary American social institutions, however, have looked quite the opposite, energetic, purposive, flexible. Genteel culture and society have never quite hung together. In this trenchant volume in the "American Century Series" by Alan Trachtenberg, they do; incorporation is the key to both.

By incorporation, Trachtenberg means not only the adoption of a corporate business system, but "the emergence of a changed, more tightly structured society with new hierarchies of control, and also changed conceptions of that society, of America itself" (pp. 3–4). The latter, the effects of incorporation on culture, is his subject. For an approach to it, he applies the anthropological concept of culture, culture as "the way of life" (p. 3). Though he is particularly drawn to "the figurative language by which people represent their perceptions" (p. 8), his study is not limited to high culture or formal expression. Nor, as with the American studies movement of the 1950s, is he centrally concerned with myths or concepts of national character.

Trachtenberg's argument is that economic incorporation dislocated American culture, generating opposing views. "The deepest changes in these decades," he writes, "lay at the level of culture, difficult for contemporaries to recognize, and baffling for historians. The deepest resistances and oppositions also lay there" (p. 7). Here, better than anywhere, is the rationale for this study. And here, too, is where questions arise.

In the course of his topical presentation, which begins with the post-Civil War West and concludes with the Columbian Exposition of 1893, Trachtenberg locates only two deep sources of resistance to the incorporation of America, Populism and working-class culture. Drawing especially on suggestive recent work by Richard Goodwyn for

the former and David Montgomery for the latter, Trachtenberg finds at least a temporary countercultural protest against incorporation, an alternative of mutuality, of communalism.

In high culture, on the other hand, he sees little resistance at all. The Columbian Exposition signifies for him a transformation of the old republican ideal into "the alliance and incorporation of business, politics, industry, and culture." Not only in the White City, Trachtenberg suggests, did high culture serve as "presiding genius" (p. 230).

Although this account may somewhat exaggerate the cultural independence of farmers and workers and understate it for intellectuals, the general picture that stands out in this provocative book is not only that high culture made it difficult "to see the rest of the world, let alone see it critically" (p. 144), but that culture at all levels was similarly, if not equally, vitiated.

Why? If the deepest resistance to incorporation lay at the level of culture, as the author contends, why was culture so ultimately ineffectual in articulating and acting upon that resistance? Although Trachtenberg repeatedly attends to the dialectic of culture and society, the vital forces explored here move overwhelmingly in one direction, not the other. Marx's feeling for the dynamic powers of capitalism as much as the methods of anthropology informs this synthesis.

JOHN TOMSICH
Reed College

ANITA CLAIR FELLMAN and MICHAEL FELLMAN. *Making Sense of Self: Medical Advice Literature in Late Nineteenth-Century America*. Philadelphia: University of Pennsylvania Press. 1981. Pp. vii, 198. \$16.50.

In this brief analysis of medical-advice literature in the late nineteenth century, Anita Clair Fellman and Michael Fellman explore the ideological content of works published during a period of unsettling social and economic changes. Unlike historians such as James C. Whorton, who have detected common themes running through much of this literature, the authors emphasize discontinuities, major shifts in concerns corresponding to socioeconomic changes. "As the social context changed," they argue, "so did advice literature" (p. 5). In particular, they argue that the optimistic faith in human perfectibility characteristic of health reformers in the 1830s and 1840s gave way after the Civil War to skepticism and despair, as middle-class Americans grew increasingly anxious about their environment and themselves. Late-nineteenth-century Americans became obsessed with "making sense of self," hoping, suggest the authors, that self-understanding and self-control

would contribute in some way toward solving the problems of society as a whole.

To guide them in their quest for self-understanding, Americans turned to a host of "instructors" and "advisors," often physicians, who taught them about sickness and health, brain and mind, will power, and sex—a subject that occupies nearly 30 percent of the book. Although the authors draw on the writings of over one hundred such advisors, they attach little weight to the individual advisor's social, medical, or religious status. "Advisors form a 'group' with shared values," they assert, "which is more useful heuristically than would be more careful divisions between regulars, irregulars, and nonmedical teachers" (p. 19). Maybe so, but I am not altogether convinced.

I am also somewhat suspicious about the degree to which the medical advice of the late nineteenth century was distinctive. The authors claim to have found substantial differences between the antebellum and postbellum periods, but their generalizations for the years 1830–50 rest almost entirely on secondary sources; only one of the nearly two hundred items in their bibliography dates from before the Civil War. If significant changes occurred, it would be desirable, I think, to trace them in the writings of individuals whose writings spanned both periods.

Making Sense of Self has significant limitations: it focuses on belief, not behavior; it pays little attention to patterns of disease and to developments in medical science; it overlooks the popular health journals of the nineteenth century, a key source of medical advice. Nevertheless, it is a provocative and at times exciting work. The authors chose to write not a definitive monograph but an exploratory essay—one that deserves to be read by all historians interested in the relationship between medicine and culture.

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ARTHUR LIPOW. *Authoritarian Socialism in America: Edward Bellamy and the Nationalist Movement*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xii, 315. \$28.50.

This book offers a new interpretation of Edward Bellamy and the "Nationalist" movement that briefly burgeoned following the publication in 1888 of Bellamy's best-selling Utopian novel, *Looking Backward*. Arthur Lipow has made no factual discoveries, and many of his interpretive insights have occurred to others. His originality is one of emphasis and conclusion. It arises from his political allegiance and

from the current widespread disillusion on the left with Soviet communism.

Lipow proclaims in his introduction his passionate commitment to democratic socialism. Socialism, as he sees it, has nothing to do with "bureaucratic statification," which he condemns along with "bureaucratic liberalism" and, of course, modern capitalism. Socialism must by definition include the widest possible political democracy as well as free trade unions. Lipow makes it clear that his political views owe much to Hal Draper, his psychological theory to Erich Fromm, and his social analysis to Richard Hofstadter.

These loyalties lead Lipow to a very dark view of Bellamy's "Nationalism." He differs sharply from the Progressive historians, who make Bellamy a part of a democratic and progressive disenchantment with laissez-faire capitalism, ancestral to later democratic reform and democratic socialism. Of socialism's "two souls," one democratic and one authoritarian, Bellamy points mainly toward the latter. He is even a precursor of modern totalitarianism, both right and left.

In Bellamy's America of the year 2000, Lipow correctly points out, the workers are organized in a hierarchical and disciplined industrial army. Dissent, political activity, and unions are all forbidden. Society is run by disinterested experts; top management is chosen by retired veterans of industry. This curious organization has been arrived at without struggle, as a natural, evolutionary consequence of the growth of monopoly. Finally the one great trust becomes the state. Capitalist rationalization and bureaucratization are crowned, but not really transformed, by state ownership.

Bellamy's vision of the future, says Lipow, appealed to members of the old, declining middle class. Appalled by the ruthless and vulgar plutocracy, frightened by strikes and mass movements, completely distrustful of the workers, some doctors, lawyers, clergymen, and others were captivated by a future in which the workers were disciplined, guided, and educated by people just like themselves. Lipow particularly dislikes in "Nationalism" and similar movements what he calls "communism," the belief in submerging the individual in some mystical whole. This was based, he says, on the inner needs of alienated individuals.

Bellamy's outlook, Lipow points out, was shared by many contemporary middle-class reformers. Some of these, he concedes, were more consistently antidemocratic than Bellamy, who moved for a short time into collaboration with the People's party and in the late nineties temporarily modified his movement in a democratic direction.

I have always found it hard to imagine reading *Looking Backward* without finding its future society dull and rigid, and its long-winded spokesman, Dr.

Leete, a superdidactic bore. Lipow explains persuasively the book's contemporary appeal. He is right in saying that some historians have explained away Bellamy's authoritarianism. As for Lipow's general message for our time, it is hard to deny that people who cannot stand the ugliness of day-to-day political and economic struggle are likely to end with something much uglier.

Where Lipow goes too far, in my opinion, is in making Bellamy and his followers ancestral in any important way to twentieth-century fascists and communists. These harmless gentlemen, with no understanding of power, squeamish about force, abhorring all mass movements, are not much like either Nazis or Stalinists. America in the late nineteenth century, however harsh and chaotic, was not a society in ideological or social breakdown like those that gave rise to the later movements. I think that Lipow might have understood the origin of the "Nationalist" upsurge better if he had given more attention to American revivalist religion, with its sudden and permanent changes of heart. Bellamy's book was written as a prophetic vision and then taken as a social blueprint by some morally and socially distressed people. Occasional exaggerations aside, Lipow's interpretation is stimulating, and those who talk about late nineteenth-century American reform will have to come to terms with it.

The book is reasonably well written on the whole, though it is marred by considerable repetition and an occasional awkward sentence.

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PAUL R. BAKER. *Richard Morris Hunt*. Cambridge, Mass.: MIT Press. 1980. Pp. xvi, 588. \$39.95.

With the Modernist Movement in architecture currently under severe attack, renewed interest in Richard Morris Hunt (1827–1895), nineteenth-century champion of eclecticism, ornamentation, and Beaux-Arts principles, seems appropriate. Happily, Paul R. Baker has now provided a detailed study of the man and his work that, with its depth of treatment and judiciousness, should gratify the architectural historian, as well as the historian of American culture.

To understand Hunt—his career, reputation, shortcomings—is to apprehend a good deal of Gilded Age artistic and social values. The first American trained at the prestigious École des Beaux-Arts, Hunt forsook an appealing cosmopolitan life and promising career in Europe to return in 1855 to his homeland, where he undertook a sort of *mission civilisatrice* to elevate both professional practice and public taste. According to Baker, the success of this

enterprise, despite limitations, constitutes one of Hunt's most important achievements. As a founder and president of the American Institute of Architects, mentor to such architects as William R. Ware, Henry Van Brunt, George B. Post, Charles D. Gambrill, and Frank Furness, and craftsman who insisted (though never so vigorously as to lose a commission) that his clients accept a properly harmonious design, Hunt became the "dean" of his profession, a true "ambassador of art."

But to paraphrase D. H. Lawrence, one must trust the building and not the builder, and it is Hunt's edifices themselves that should, and do, engage the author's primary attention. For his critics Hunt was, as it were, the unofficial house architect to Gilded Age entrepreneurs. Although the great New York mansions built for various scions of Commodore Vanderbilt no longer exist, other palatial residences, including The Breakers (Newport) and Biltmore (Asheville), stand today as reminders of the grandeur and arrogance of the captains of industry. Derivatively styled, exquisitely executed, they reify the Veblenian emulative desires of this culture-conscious business elite and the artist who contended with their whims.

Yet Hunt neither toadied to the rich nor restricted his artistic purview to the private sphere. Although he usually did obtain commissions through the favor of wealthy patrons or influential friends rather than through open competition, his achievements, varied and profuse, won widespread critical and popular acclaim. His Fifth Avenue wing of the Metropolitan Museum of Art, pedestal and base for the Statue of Liberty, and administration building for the Columbian Exposition speak to the point. Post-Civil War taste leaned toward the massive and the traditional, and in Hunt found its avatar, one who preferred "dignity and repose" to "eccentricity" (p. 330). Ultimately rejecting the denigrations of both social critics and critics loyal to the dictates of modernism, Baker takes Hunt on his own terms, judging him as a man of his times who, though flawed, was a considerably finer, more important artist than generally acknowledged.

This study, to be sure, presents moot points. Although an impressively researched biography, it passes rather too quickly over certain events: the effect on Hunt of his father's early death, the suicides of two brothers (one of whom was the noted painter, William Morris Hunt), Hunt's failure to attend, as he had hoped, West Point or serve in the Civil War, even the exact cause of his death. More important, while contemporary architects are frequently cited, greater attention might have been devoted to Hunt's estimate of such confreres as Richard Upjohn, Henry Hobson Richardson, Charles McKim, Stanford White, and Louis Sullivan, and vice versa. Still, these points diminish in

face of Baker's masterful overall achievement. Possessing depth of architectural analysis, historical texture, numerous fine illustrations, and a complete list of the subject's buildings, and written with the "dignity and repose" that its subject so admired, *Richard Morris Hunt* is a scholarly tour de force and a model of wholesome revisionist writing.

ROBERT MUCCIGROSSO
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EUGENIA KALEDIN. *The Education of Mrs. Henry Adams*. (American Civilization.) Philadelphia: Temple University Press. 1981. Pp. xvii, 306. \$22.50.

This is the second recent biography of Marian Hooper Adams—"Clover," wife of Henry Adams—the other being Otto Friedrich's *Clover* (1979). Unsurprisingly, today's attention to women's role in history has redoubled interest in Mrs. Henry Adams, whose life and suicide made her story one of America's most intriguing situations. Eugenia Kaledin's search for an authentic Clover is, in my opinion, more successful than Friedrich's, but ultimately it, too, must stand apart from Clover and Henry who were determined that their lives remain out of reach. Henry Adams tantalized future generations by omitting any reference to Clover and their marriage in his *Education*. Clover even destroyed all useful photographs of herself, and the manuscript material about her is scant, save for letters to her father. Despite this handicap, Kaledin manages to write a book-length study, largely by emphasizing Clover's background and times. When approaching Clover herself, Kaledin wisely cautions the reader by frequent use of "it seems," "may," and "perhaps."

The author's best work is in describing Clover's surroundings, except for the Adams family whose nature, including that of Henry Adams, eludes Kaledin. She sees the Adamses as villains, characters that were cold, self-centered, abrasive, and sexist. As the mass of Adams family manuscripts shows, the Adamses were far more complex than Kaledin's version. Most serious is her failure to see Henry Adams as he was, a gentle, loving person who strove mightily to care for Clover, and who understood her disease. Most astonishing is Kaledin's description of Henry's mother, Abby Brooks Adams, as "brilliant" (p. 244), perhaps the least appropriate adjective that comes to mind.

It was into the Adams family that twenty-eight-year-old Marian Hooper married in 1872 after a brief romance with Henry, whose startled relatives had been convinced he was not the marrying sort. The groom heard numerous warnings from others that Clover was likely to inherit a mental pathology tending toward suicide. Henry said he knew the

risks he was taking. While Kaledin briefly acknowledges that Clover's outlook and behavior could have been caused by biochemical flaws, she prefers to argue that Clover's famed irreverence, bitterness, restlessness, and depression were attributes of a vigorous person whom society and marriage suppressed. In 1877, Henry had taken Clover from Boston to Washington to live, hoping that in the new setting her sharp tongue would be less noticeable and that her sense of worthlessness might be overcome. The change parted Clover from her father, Dr. Robert Hooper, who proved to be her mainstay in life. Her mother, Ellen Sturgis, who had died prematurely, was from a lineage notable for emotional instability. The Sturgis women were remarkably gifted, frail souls, poetically inclined and attracted to Margaret Fuller and the Transcendentalists. Kaledin presents Clover as another of these talented women kept from fulfillment. Thirteen years after her wedding, in December 1885, the dire prophecy was fulfilled and Clover poisoned herself. Her sister destroyed herself in 1887, and thereafter her brother died in the aftermath of a suicide attempt.

The strongest part of Kaledin's work is her treatment of Clover's mother, other female relatives such as Carrie Sturgis Tappan, as well as peripheral figures in Clover's world, including Henry James, R. W. Emerson, Elizabeth Bancroft, and Margaret Fuller. Much of this success is due to Kaledin's skill in using manuscripts still in the possession of Faith Knapp, granddaughter of Clover's brother. Many of these papers have until now been withheld from scholars. They are not, however, very helpful in the search for Clover. Today, as in her time, no one understands the anguished psyche that caused Clover's sympathetic father-in-law, Charles Francis Adams, to refer to her sadly in his diary as "a marplot and a subject of commiseration." *The Education of Mrs. Henry Adams* makes Clover a cause rather than a person, for Clover left us so little concerning herself that we cannot even say with assurance what she might have thought about Kaledin's use of her story.

PAUL C. NAGEL
Virginia Historical Society

LINDA O. MCMURRY. *George Washington Carver: Scientist and Symbol*. New York: Oxford University Press. 1981. Pp. x, 367. \$25.00.

It is always somewhat depressing to read biographies of Afro-Americans, and the longer the person lived, the more depressing. This is because such works tell a great deal about the structure of the society and the extraordinary extent to which members of the dominant society have gone in maintain-

ing the oppression of people of color. This society appears to have a special penchant for outrageous contradictions when it comes to black people.

Having been born a slave, George Washington Carver grew up in a largely white environment. He attended a small college and later studied agriculture at Iowa State College. Invited to join the staff of Tuskegee Institute by Booker T. Washington in 1896, Carver immediately commenced experiments on many foodstuffs. Ultimately, however, he focused his attention on three crops: cowpeas, sweet potatoes, and peanuts. His purpose was to change the character of Southern agriculture, and, above all, to help the "man furthest down."

In the classroom at Tuskegee he was an excellent, thoughtful, and inspirational teacher. But throughout Washington's presidency his arrogance was apparent in relationship to Carver. He was never paid adequately, and it is nothing short of amazing how many different projects he initiated with virtually no assistance. In spite of these difficulties, Carver became one of the world's best-known scientists, admired by people everywhere.

International fame or not, he was the victim of many segregationist practices. For example, he was not permitted to take Pullman accommodations, and when he arranged for a fair to be held in Tuskegee he was required to provide segregated facilities.

Linda O. McMurry has written a sympathetic and thoughtful biography of George Washington Carver. He is sometimes portrayed as a tattered old man who knew little about chemistry or nutrition; indeed he is not seen as a scientist at all. At other times he is seen as a Christ-like figure, charming all he met. His network of friends was vast. For McMurry he may be seen as a pioneer in solar energy and organic farming, neither of which was highly developed in his lifetime. His attempt was to develop programs that would serve both people and the environment.

This is a valuable study of one of the century's most amazing men.

ALPHONSO PINKNEY
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City University of New York

DEBORAH DASH MOORE. *B'nai B'rith and the Challenge of Ethnic Leadership*. (SUNY Series in Modern Jewish History.) Albany: State University of New York Press. 1981. Pp. xvi, 288. \$18.95.

B'nai B'rith is the oldest secular Jewish organization in the United States and as such its history is as diverse as that of American Jewry itself. It was founded in New York City in 1843 by German-Jewish immigrants as a fraternal order committed to

humanistic Judaism and ethnic secularism, a "secular synagogue" as Deborah Dash Moore denotes it. Consequently, the Order of B'nai B'rith acknowledged all Jewish religious identifications and sought in difficult times to bring them into harmony, not always with much success. It grew with the Jewish population and in 1883 even extended abroad, making B'nai B'rith perhaps the first indigenous Jewish-American organization to export its approach overseas.

B'nai B'rith's physical and demographic expansions provided the foundation for the broad scope of its activities in the twentieth century. This study focuses on these institutional changes and the communal conflicts that influenced their development, and in the process reveals much about the varied organizational and social history of the Jewish-American community. Moore does a credible job of describing how B'nai B'rith moved into the realm of philanthropy, conducting programs and projects in Israel of a philanthropic and educational nature and supporting several national Jewish hospitals and homes for the aged; the area of defense, establishing the Anti-Defamation League in 1913, one of the most important Jewish-American defense organizations; and the spheres of education and service, forming the Hillel Foundations, an international network of student groups on college campuses, the Adult Jewish Education Commission, and the B'nai B'rith Youth Organization, all with the goal of achieving leadership in the Jewish-American community.

The volume was commissioned by B'nai B'rith at the approach of the U.S. bicentennial. On the whole, Moore maintains proper scholarly distance and independence and presents a fairly insightful analysis of the organization, its internal developments and conflicts, its leadership, and its role in the Jewish-American experience. One gets the sense, however, that the author is writing for two audiences—the lay supporters of B'nai B'rith and the scholarly community. At times Moore does not succeed in her attempts to achieve a popularized version of a scholarly endeavor. In several chapters the historical detail and documentation are difficult to get through and the excitement of insight provided by original synthesis of materials is missing. This is not a seminal work, as it relies too much on secondary literature and is occasionally trite and predictable. Yet it goes beyond the works of Adolph Kraus, Maurice Bisgyer, Edward Grusd, and Nathan Belth and provides the best opportunity to date for understanding B'nai B'rith by relating it to the historical forces shaping the American and Jewish-American communities.

MICHAEL N. DOBKOWSKI
Hobart and William Smith Colleges

GARY DEAN BEST. *To Free a People: American Jewish Leaders and the Jewish Problem in Eastern Europe, 1890–1914*. (Contributions in American History, number 98.) Westport, Conn.: Greenwood Press. 1982. Pp. xi, 240. \$27.00.

Ten years ago Gary Dean Best published a fine article on Jacob Schiff's financial help to Japan during its war against Russia. But like swallows and spring, one article does not make a good book. Best's chronological survey of American Jewish political lobbying on behalf of persecuted Jews in Eastern Europe between 1890 and 1914 is overshadowed by grave shortcomings.

Except for supplementary material unearthed in the Schiff papers, the author breaks little new ground. Others before him—often with greater skill—have told the stories of the Jewish response to the Kishinev pogrom, the fight to abrogate the Russo-American treaty of 1832, and Jewish efforts against immigration restriction. Although Best has researched important manuscript collections and government records on those issues, one may legitimately question the rectitude of redoing the spade-work of older monographs when no substantive revisions emerge. Those acquainted with the secondary literature may well wonder whether Best would have arrived at both facts and conclusions without the help of the footnoted road maps provided by the earlier literature. His use of an article on the abrogation of the Russo-American treaty is also troublesome. Best implies that the article overlooked the prime goal of the Jewish lobbyists, which he then develops as his own interpretation but which is actually the thesis of that very article.

Best strings together the ad hoc responses to the crises besetting Romanian and Russian Jewry under the shaky thesis that they significantly shaped what became "the most powerful and efficient foreign policy lobby in American politics." (The quote, accepted at face value, is from the less than dispassionate Senator William Fulbright.) Even if one went along with that premise, he could rightfully expect an in-depth analysis of the multiple motives prompting the lobbyists, the intricacies of their teamwork, the reasons why they were heeded by power brokers, and the differences between those leaders and the premodern "court Jews." The book also ignores concerted Jewish attempts to influence legislation on immigration from the creation of the Dillingham commission until the outbreak of the war.

The author dismisses humanitarian diplomacy on the part of the American government as mere window dressing for political favors to ethnic groups. An awareness of the universalist element in American ideology, however, which surfaced in early nineteenth-century reactions of sympathy for

oppressed peoples, might have modified his assumption. Best completely ignores the example of the Damascus affair of 1840, when the Van Buren administration intervened on the side of Ottoman Jews accused in a blood libel even before American Jews called it to the government's attention. On the other side of the coin, the author's case is incomplete without a study of Jewish voting patterns revealing whether the community returned political favors at the polls.

Best deals with an era that witnessed a sharp rise both in American antisemitism and immigration restrictionist sentiment. The conflict between the needs of the European Jews and the American temper spawned new difficulties for the accommodationist Jewish leaders and their political strategy. By neglecting that dimension, the book misses still further the full drama of the critical age of Jewish stewardship.

NAOMI W. COHEN
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City University of New York

ROGER M. OLIEN. *From Token to Triumph: The Texas Republicans since 1920*. Dallas: SMU Press. 1982. Pp. x, 309. \$15.00.

This finely tuned account of recent Texas Republicanism traces party developments from 1920 when the GOP candidate for governor received a mere token vote through 1978 when Texas re-elected John Tower to a fourth term in the U.S. Senate and for the first time since Reconstruction sent a Republican, William P. Clements, Jr., to the governor's mansion. In those fifty-eight years the Texas Republican party grew from a shell of an organization headed by Rentfro B. Creager, a Brownsville lawyer, for the primary purpose of dispensing federal patronage and sending favorable delegations to Republican national conventions into an election-winning machine. Roger M. Olien follows the ups and downs of Republican fortunes in the state with scholarly precision and a style that invites a wide readership. He has drawn on manuscript collections, interviews, newspapers, and other sources to mold a lively study of Texas Republican politics.

After years of tokenism under Creager, who generated the Texas party from 1923 until his death in 1950, the real story of election-winning Republicanism in the state started in the early 1960s. Olien has a full account of the role played by Texas in the Eisenhower landslides of 1952 and 1956, but those victories were primarily the work of Democrats who affiliated with the GOP to elect a popular war hero with conservative credentials. Though Texas Republicans profited little from the Eisenhower years,

things picked up from 1960 onward. Richard M. Nixon came within forty-six thousand votes of winning Texas out of more than two million cast in his tilt with John F. Kennedy, and then John Tower grabbed the Senate seat held formerly by Vice President Lyndon Johnson in a 1961 special election. A temporary setback accompanied the Goldwater fiasco, but the party had a grassroots organization in place for future victories.

The author does a good job in detailing the new Republicanism that grew out of an expanded middle class and increased migration by Eastern and Midwestern Republicans into the state's urban centers. Concurrently, a conservative bent developed among Texas Democrats as their party drifted toward a liberal stance on national economic and social issues. Big oil operators in the state found the GOP an increasingly attractive vehicle for campaign contributions as the Democrats moved to what they perceived as an anti-private enterprise position. The book not only describes the new political conservatism in Texas and its relation to developments throughout the South, but it also contains useful accounts of down-home careers of major contemporary politicians such as John Tower and George Bush.

But it was Texas Republican women who carried the party to victory; though the men put up the candidates, it was the women who won elections through hard-driving organizational tactics. They used everything from massive telephone campaigns to fund-raising teas to get out the vote for party candidates. Republican women undoubtedly played a key role in fashioning the party into a successful organization, and Olien's account of their activities starting with the first Eisenhower election deserves a wide hearing.

PAUL D. CASDORPH
West Virginia State College

ROGER M. OLIEN and DIANA DAVIDS OLIEN. *Oil Booms: Social Change in Five Texas Towns*. Lincoln: University of Nebraska Press. 1982. Pp. xiv, 220. \$16.95.

In the twentieth century, oil has replaced gold, silver, cattle, and timber as the principal base for Western American boom towns. *Oil Booms* is a systematic description of the impact of rapid growth and economic consolidation in five towns and cities in the Permian Basin of west central Texas. Using 1920 census data as a baseline, the analysis starts with the drilling boom of 1926-27 and carries through the 1950s.

The five towns fall into three types. Midland and Odessa were farm-market centers transformed by the series of booms into substantial regional centers.

They have grown from a combined population of 2,749 in 1925 to more than 180,000 in adjacent metropolitan areas in 1980. Snyder was another farm-market center that enjoyed new growth on a solid economic base after 1949. McCamey and Wink were classic boom towns, exploding from lonely crossroads or railroad sidings into towns of several thousand in less than two years during the late 1920s.

Roger M. Olien and Diana Davids Olien undertake to refute the "mud and blood" stereotype of Southwestern oil towns, using published census data, town records, and interviews with participants in the booms. They describe sources of boom-town population, the conditions of housing, the difficulties of providing water and schools and disposing of garbage and sewage, and the interrelations between crime, law enforcement, legal, and illegal amusements. Some of the most interesting discussion deals with subdivisions within the boom-town labor force—the differences among categories of oil-field workers and the job opportunities for women, blacks, and Mexican-Americans.

One contribution of the study is the assembly and summary of reliable data on communities that have received little attention from social scientists. Another is an analysis of ways in which oil booms differed from previous mineral booms. The explosive population growth during the labor-intensive stages of exploration and drilling was similar to the swarming of prospectors who crowded mountain valleys in the first year or two after a new gold or silver strike. In the production stage, however, oil fields used far fewer workers than hard-rock mines and were less able to sustain long-term urban growth. By implication, Rocky Mountain states currently experiencing intensive oil exploration would be advised to look to twentieth-century Texas and Oklahoma rather than to their own history for models of social and economic impact.

The book is weakest in placing its substantial research into context, whether of Western history, community studies, social theory, or growth impact assessment. References to historical studies are thin, and to other literature almost nonexistent. For example, sources as disparate as Robert Dykstra's *The Cattle Towns* and studies of recent oil booms from Wyoming to Scotland stress internal conflicts and differential economic impacts within boom towns. The authors briefly mention but fail to explore a number of such conflicts whose treatment would have deepened understanding of their chosen topic of "community life." Fuller use of theoretical literature also might have led to more systematic discussion of a point implied in the narrative—the different problems involved in creating a new community (Wink, McCamey) and expanding an existing one (Midland, Odessa, Snyder). Within these

limits, however, *Oil Booms* usefully carries a major theme of Western American history from the nineteenth century through the middle of the twentieth.

CARL ABBOTT

Portland State University

JAMES C. FOSTER, editor. *American Labor in the Southwest: The First One Hundred Years*. Tucson: University of Arizona Press. 1982. Pp. xii, 236. Cloth \$18.50, paper \$9.85.

The articles gathered in *American Labor in the Southwest: The First One Hundred Years* address both a broader and narrower subject than the title implies. Essays dealing with the labor movement in Alaska, Hawaii, and Mexico, as well as two works with a national scope, stretch the focus of this book far beyond the traditional Southwest. At the same time, however, "labor" is given a restricted definition. Histories of unions' organizational and political activities preempt any examination of other aspects of laborers' culture. Moreover, the conservative craft organizations that dominated the union movement until the 1930s are almost completely ignored in favor of the more radical unions.

Space permits discussion of only a few of the fourteen contributions in this volume. James C. Foster, the book's editor, reveals a relatively peaceful and moderate Western Federation of Miners in two articles studying the WFM's local organizations. Yet his intriguing articles are only suggestive; the first because it deals with too small a segment of the WFM and the second because the methodology of its quantitative analysis of all locals is inadequately explained and because it attempts to collapse the history of twenty-seven years of a movement into a set of statistics that ignore temporal changes in economic, social, and political conditions.

A pair of articles grapple with the development of the Mexican labor movement. John M. Hart gives a useful outline of its history from the 1860s through the 1920s emphasizing the conflict between moderate unionists whose efforts the government supported and radical unionists whose activities were suppressed. He highlights the radicals' role in leading workers in the revolution of the 1910s. In contrast, Rodney Anderson's essay assails the theory of radical leadership in the revolutionary era as a myth. He argues that the radical Mexican Liberal Party (PLM) capitalized on but did not lead workers' protests and that the Mexican workers rejected the class-consciousness preached by the PLM.

D. H. Dinwoodie's examination of the Mine, Mill, and Smelter Workers Union, James Byrkit's vivid description of the 1917 Bisbee deportation, Edward D. Beechert's history of agricultural unionization in Hawaii from the mid-1930s to 1946, and George N.

Green's study of labor baiting as a political tactic of the Texas "Establishment" all deserve favorable mention. Charles O. Rice's *mea culpa* regarding the CIO purge of 1948 is eloquent, if not particularly illuminating.

Nevertheless, this book lacks focus and boldness in interpretation. Hart describes the interplay between the Mexican government and unions, but neither he nor the editor offer any thoughts on how this historical background may have affected unionization of Mexican-Americans in the American Southwest. Beechert and Paul Mandel, whose article discusses Senator Carl Hayden's early prolabor politics, would have done well to place their studies in a larger context to extract insights of broader significance. And most surprisingly, nowhere does the book seriously address the question of what made the labor movement in the Southwest unique, although Foster is convinced that there "must have been something peculiar indeed with the sky, the air, the water" (pp. 8–9) in the West to produce the radical labor movement portrayed in this volume.

JAMES H. DUCKER
Bureau of Land Management
Anchorage, Alaska

WILLIAM L. KAHL. *Water and Power: The Conflict over Los Angeles' Water Supply in the Owens Valley*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xii, 583. \$24.95.

For many decades the dispute between the city of Los Angeles and the Owens Valley in Inyo County, California, over the city's acquisition and use of water and real estate in the valley, has continued—at times violent, other times simmering, and constantly litigious. Questions of conspiracy, fraud, public malfeasance, and private greed have made this controversy a popular topic for novels and motion pictures—most recently the 1974 film, "Chinatown." Oddly enough, professional historians long left examination of the controversy to polemicists, biased journalists, and generalizing popularizers who wrote undisturbed by their errors of fact and self-interested judgments. Only two studies of note appeared on the subject before the 1970s—Remi Nadeau's unfootnoted *Water Seekers* (1950), written for a general audience, and Vincent Ostrom's *Water and Politics* (1953), a disorganized hodgepodge of facts based on published sources.

All this has been changed, thanks to recent studies that have used important manuscript sources at the National Archives, the National Personnel Records Center, and other repositories. William Kahl's book stands at the forefront of the recent research. It is ambitious, comprehensive, and grounded in those long-neglected sources historians could have used

all along instead of relying on the same biased secondary works. Kahl considers the political, economic, and environmental consequences of the city's use of a water supply located some two-hundred-fifty miles away and its aggressive defense of this source for its own growth and prosperity. His work is for the most part persuasive; historians generally familiar with the outlines of the controversy will be impressed with the quality and scope of the research. Specialists, however, may question some of Kahl's judgments on past events. Kahl prefers the erratic Stafford W. Austin's statement to Special Inspector S. F. O'Fallon, rather than J. B. Lippincott's private letters and telegrams to Fred Eaton, in the matter of whether the U.S. Reclamation Service abetted Eaton in his purchases of water rights secretly on the city's behalf—a choice with which this reviewer must disagree, as is also the case with Kahl's taking at face value the writing of polemicist Andrae B. Nordskog. Lippincott is very much the villain of the piece: Kahl finds him capable of "character assassination" (p. 22) in attacking George Chaffey; he is the "betrayal of the valley" (p. 291); and the one who "alone consistently broke faith with his public trust and then lied to cover his actions" (p. 439). These are harsh judgments by Kahl, particularly since he ignored the Lippincott-Newell correspondence at the Library of Congress, letters that give quite another perspective on Lippincott's actions.

There are other interpretations subject to debate: Kahl states the Department of the Interior's investigation into Lippincott's conduct was decided in the spring of 1906 (p. 128) rather than February—seemingly a minor point, except that it conveys the impression Lippincott resigned immediately after the conclusion of the investigation, not five months later. But these are the quibbles, the fine-tuning that will now be possible thanks to enlargement of the topic through Kahl's presentation. In carrying his account down to the present, Kahl provides the best coverage yet available of the complexities of the so-called water wars of the 1920s, and he does the same for succeeding decades down to the 1970s. And yet, despite the depth of research and the perceptive analysis, there are still unanswered questions: Where were the members of the Board of Water and Power Commissioners? Were they merely rubber-stamping Mulholland's and his successors' policies? John R. Haynes is barely mentioned by Kahl; Reginaldo del Valle, a commissioner for twenty-one years, is not mentioned at all.

As for the publisher's assertion that the work is "definitive," Kahl himself notes that his book "is scarcely the last word on the Owens Valley conflict: the final chapter, after all, has yet to be written" (p. x). Kahl invoked the same sentiment when, concluding his paper at the 1981 American Historical

Association meeting, he invited historians to join him in further research into the topic, urging, "Come on in, the water is great." The Owens Valley-Los Angeles water controversy historiography will never again be the same.

ABRAHAM HOFFMAN
Los Angeles Valley College

RONALD F. LOCKMANN. *Guarding the Forests of Southern California: Evolving Attitudes Toward Conservation of Watershed, Woodlands, and Wilderness*. (Western Lands and Waters, number 12.) Glendale, Calif.: Arthur H. Clark. 1981. Pp. 184. \$19.50.

The title of Ronald F. Lockmann's *Guarding the Forests of Southern California* promises more than the text delivers. As did the doctoral dissertation from which it derives, this work deals only with the Transverse Ranges that stretch eastward from near Santa Barbara, along the north edge of the Los Angeles basin, to the Mojave Desert. North-reaching extensions of the peninsular ranges of Baja California, and their forests, go largely unmentioned. A map captioned "State Population Centers in Relation to Southern California National Forests" (p. 18) omits the Cleveland National Forest, thereby giving the impression that the closest forests to San Diego are some one hundred miles away; and the map titled "Wilderness Allocation, 1980: Southern California National Forests" (p. 160) omits Agua Tibia wilderness, located south of the Transverse Ranges. Conversely, Lockmann's map "Density of Forests, 1884: Transverse Ranges, California" (p. 52) includes *all* forested areas in southern California. The confusion of purpose thus illustrated also permeates the text.

Lockmann traces the growing knowledge and use of the Transverse Ranges from 1790 to 1980. He discusses the impact of federal legislation, the establishment of forest reserves (later called national forests), and shifting land use. Sparsely timbered, these forests have been valued primarily for watershed protection, grazing, and recreation. In spite of limited commercial stands, lumbering was also practiced. A chapter is devoted to each use.

The author's analysis is disappointing. Discussion of land policy is dated and unoriginal. His chapter on the establishment of forest reserves cites Harold K. Steen's *The U.S. Forest Service* (1976), but fails to incorporate key information from it. His surveys of watershed protection and recreation include little not already familiar. Lockmann's main contribution—information on Theodore P. Lukens—has already appeared in his article in the *Journal of Forest History* in 1979.

There are additional problems. Scientific names are inconsistently rendered, common names inaccurately

used. The southern limit of the coast redwoods is mislocated. The leasing of grazing lands by forestry officials is placed too early, and the purpose misrepresented. The Forest Reserve Act of 1891 did not create reserves, nor was there a national park administration in the nineteenth century. The transfer of forest reserves from the Department of the Interior to the Department of Agriculture is misdated. And at one point he doubts, at another accepts, lumber production figures for 1873. On another level, Lockmann uses individual species to illustrate the problems of ecosystems. He says that wilderness is by definition unused but then proceeds to discuss its recreational use. More centrally, he fails to note the declining importance of watershed protection as southern California shifted to imported water. This shift and the declining role of grazing and logging have profoundly affected management. Finally, a geographer from California ought to know better than to place Owens Valley "in the Sierra Nevada" (p. 108) or to call the range "the Sierras" (pp. 118, 150).

This is a handsome book that careful editing might have improved. More than that, however, would have been needed to redeem it.

THOMAS R. COX
San Diego State University

ROBERT M. COLLINS. *The Business Response to Keynes, 1929-1964*. (Contemporary American History Series.) New York: Columbia University Press. 1981. Pp. xii, 293. \$24.00.

Robert M. Collins's theme is the role businessmen played in determining that Keynes's ideas would be applied, in postwar America, not according to the plans of the stagnation theorists but to an extent and in a manner least disturbing to businessmen's independence and to the existing social and economic order. Collins traces in painstaking detail the ideas and influence of the traditional organizations, the NAM and the Chamber of Commerce, but emphasizes the role of the Council for Economic Defense. The CED, organized during World War II by a number of more flexible business leaders, played by far the most important role in adapting Keynes's prescriptions to fit business needs. The CED's leaders realized that a compromise with deficit spending somehow was necessary if business was to escape from the merely negativistic criticism that had cost it most of its influence with the Roosevelt administration during the late 1930s. Compensatory spending through tax reductions (rather than through increased discretionary spending) thus became the fiscal program of the CED to correct unemployment during recession and, as eventually formulated, even to solve the problem of retarded economic

growth. Collins traces the influence upon public policy of the CED's position (which was gradually reflected by the Chamber), arguing that the newly flexible business proposals were a major factor in the fading away as a serious policy alternative of stagnationism, whose version of Keynes's ideas called for continuous, large-scale compensatory spending, coupled with redistributionism and reformist policies, on the basis that American capitalism left to itself would founder. Collins ends with the passage of the Kennedy tax cut, demonstrating that business support was influential in formulating and passing the proposal, just as it had been in determining the fiscal policy of the Eisenhower years.

Collins's book has many strengths. He argues effectively that increasing use of and cooperation with professional economists by the CED and the other business groups was an important innovation that greatly increased business credibility. His description of the educational-propaganda techniques the CED used to spread its message is thorough and interesting. His explanation of why the CED's response was so much more adaptive than the response of traditional business groups to the institutional-political situation the New Deal had created, and so much better able to restore business influence in public councils, is useful, and his sketches of the personalities and motives of key figures is revealing.

One may question, however, whether Collins's central conclusions are sustained as completely as would be desirable. One major conclusion, that business played a "significant part in redefining Keynesianism for the post-war world" is certainly firmly established, but it is perhaps legitimate to suggest that Collins should have given more consideration to the context of the postwar epoch, and to the questions. How significant a part? How much did the adoption of a conservative, investment- and growth-oriented version of Keynes owe not to businessmen but to the search of liberal and moderate opinion for policies consonant with the postwar economy and public mood?

A second major conclusion strikes this reader as more problematical. Collins finds these adaptors of Keynes in the "corporatist" tradition but includes little account of the ideas or policy goals of his business figures except with respect to their creation, "commercial Keynesianism," whose purpose, as Collins explains, was to prevent intrusive statism. Maintaining freedom from statist controls does not itself place these businessmen in the corporatist tradition as usually defined (and as Collins himself, who is thoroughly familiar with the literature of the subject, describes it). Thus it seems questionable whether Collins has advanced very far our capacity to judge the relationship of postwar business ideolo-

gy and policy advocacy to the stream of corporatism that extends from the Morgan detente system to the NRA and post-NRA formulations. Collins of course has chosen his subject, business thought on fiscal policy, and himself notes that similar studies need to be done on other aspects of business policy and propaganda in recent times. My point is that it is difficult to characterize persuasively business attitudes as to underlying meaning until such studies have been done.

This is a thoroughly researched and often highly original work, however, that does illuminate the thought and influence of businessmen in the formation of public policy and opinion in the post-1945 epoch.

ROBERT F. HIMMELBERG
Fordham University

JOHN W. JOHNSON. *American Legal Culture, 1908–1940*. (Contributions in Legal Studies, number 16.) Westport, Conn.: Greenwood Press. 1981. Pp. x, 185. \$23.95.

John W. Johnson's slender volume promises more than it delivers. Instead of a grand synthesis along the lines of Willard Hurst's *Law and the Conditions of Freedom in Nineteenth-Century America* or Grant Gilmore's *The Ages of American Law*, Johnson has reduced the nation's legal culture in the years from 1908 to 1940 to a single process: the triumph of what he calls "the informational mind-set in American law," or what might be defined more simply as the accession of the Brandeis brief and the Brandeis judicial opinion.

Prior to the twentieth century, Johnson argues, the American legal system remained largely self-contained and grounded in the elaboration of immutable, a priori principles. After the turn of the century it became fact oriented and subject to the influence of other academic disciplines, especially the new behavioral sciences of sociology, psychology, and economics. Sociological jurisprudence and legal realism replaced legal philosophy as the dominant intellectual paradigm.

The broad outlines of Johnson's thesis are not new, having been developed long ago by Morton White in his *Revolt Against Formalism* and elaborated upon by G. Edward White and other legal historians. Johnson's book adds a few touches to this standard portrait, especially in its discussion of curriculum reforms at Columbia, Johns Hopkins, and Yale during the late 1920s and early 1930s, but the largest flaw in this work is the author's idea that a single important innovation in legal reasoning constitutes the transformation of an entire legal culture.

A legal culture, especially the legal culture of a

modern, industrial society such as the United States, is a complicated network of ideas, institutions, and people that cannot be summarized with a few hackneyed clichés. At a minimum, the discussion of a nation's legal culture ought to include an analysis of formal legal doctrines and their transformation over time as well as some inquiry into the conflict between folk attitudes about the law and those held by professionals. A legal culture also includes formal institutions such as courts, legislatures, administrative agencies, law schools, and professional associations. Johnson pays very little attention to many of these aspects of American legal culture or to their relationship during the period from 1908 to 1940.

By focusing his argument upon constitutional litigation, Johnson also tends to exaggerate the triumph of the fact-laden Brandeis brief and Brandeis opinion. One would like to know whether this same innovation took place with respect to private law disputes in the areas of torts and contracts. Even with respect to constitutional law, the case is not open and shut. Brandeis's innovations took place during the high tide of mechanical jurisprudence that still dominated the Supreme Court until 1937. And Brandeis's closest ally on the court, Justice Holmes, bore considerable intellectual resemblance to the older school of jurisprudence. The history of American legal culture in the first half of the twentieth century is not as tidy as Johnson would have us believe.

MICHAEL E. PARRISH
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San Diego

BRUCE ALLEN MURPHY. *The Brandeis-Frankfurter Connection: The Secret Political Activities of Two Supreme Court Justices*. New York: Oxford University Press. 1982. Pp. x, 473. \$18.95.

This monograph on the extrajudicial activities of two renowned Supreme Court justices stirred national discussion upon its appearance in the spring of 1982. It revealed that Justice Louis D. Brandeis between 1916 and 1938 paid Professor Felix Frankfurter some fifty thousand dollars in expense money to promulgate his views, and that Frankfurter subsequently as a justice was even more active politically than Brandeis had been. Friends and former clerks of the justices defended them; Roy Cohn, a one-time assistant to Senator Joseph McCarthy, denounced Frankfurter as a "\$50,000 political pimp."

Bruce Allen Murphy has contributed much fuel to a long-standing controversy over whether assuming the judicial robe should require political monasticism. He has written a valuable study despite

shortcomings—a sprinkling throughout both of errors and of self-congratulatory accounts of his discoveries—delineating in detail the systematic way in which Brandeis and Frankfurter sought to influence public policy. To historians at all familiar with the two justices, the revelations will not seem as startling as they did to the press. Yet the views of the two men and their efforts to win acceptance for them have never been so searchingly studied and evaluated.

Throughout the history of the Supreme Court, as Murphy summarizes in an appendix, a number of justices had been active outside the court, but by the Progressive era the ideal had become noninvolvement. Brandeis, who had been ardent on behalf of decentralization and other reforms, took very seriously his official withdrawal; he would not even accept honorary degrees. (An exception was wartime when he was fairly openly in notable service to President Wilson.) If, however, his hand were not seen, he felt he could ethically promote good causes. His vehicle was Professor Frankfurter of the Harvard Law School. Since Frankfurter did not have a large income, Brandeis persuaded him to accept an annual stipend for postage and phone bills. Later, in the 1920s, Brandeis increased it to help meet Mrs. Frankfurter's psychiatric expenses. The sums were small compared with what Frankfurter could have earned as a legal consultant. Nor was this kind of underwriting unusual; Joseph P. Kennedy in 1933 offered to aid the chief braintruster, Raymond Moley. (Moley declined.)

What was phenomenal was the systematic industry with which Brandeis fed suggestions to Frankfurter who then acted upon them with diligence. It was Frankfurter who, without citing Brandeis, wrote and telephoned on behalf of Sacco and Vanzetti and countless other causes, who saw to it that Brandeis's observations appeared anonymously in the *New Republic*, and who at Brandeis's suggestion assigned law students to research and publish in the *Harvard Law Review* critiques of the Supreme Court's work. Too, each year Frankfurter sent Brandeis a law clerk.

When Franklin D. Roosevelt became president, Frankfurter and Brandeis focused their efforts on him, succeeding in replacing the so-called collectivist program of the Hundred Days with the decentralized, regulatory Second New Deal. As Murphy grants, this is a view of the New Deal not every scholar would accept. Also it exaggerates the influence of Brandeis, Frankfurter, and their coterie. The nature of that influence was well known at the time; Murphy's contribution is to fill in the pattern. Frankfurter's success came through his incessant sycophantic approach to Roosevelt and his placement of numerous brilliant young men in strategic New Deal positions. Benjamin Cohen and Thomas

Corcoran became two of the most significant drafters of legislation and presidential advisers. Acting out of principle, Brandeis firmly opposed Roosevelt's 1937 court reform plan. Frankfurter's loyalty to Roosevelt had become greater than to Brandeis, and the long-standing alliance dissolved.

A new phase began in 1939 when Roosevelt elevated Frankfurter to the court. "When a man goes on the Supreme Court he permanently takes the veil," Frankfurter had written earlier, and he went through the outward motions of doing so. Privately, as was notorious at the time, he sought incessantly to influence both appointments and policies, especially during World War II. Nor was he alone. Other Roosevelt appointees to the Supreme Court frequently advised him or undertook important assignments.

In the context of the times, nothing that Brandeis and Frankfurter undertook was clearly unethical, although even then exposure would have created a sensation. Standards have been more exacting in recent years. As for their achievements, although they were significant, they were far from unique. The Brandeis-Frankfurter influence was one among diverse pressures on President Roosevelt, to which he gave heed only when it suited his purposes.

FRANK FREIDEL
University of Washington

MICHAEL E. PARRISH. *Felix Frankfurter and His Times: The Reform Years*. New York: Free Press or Collier Macmillan, London. 1982. Pp. vi, 330. \$17.95.

With regard to research in the fields of public law and American constitutional history, the period 1981–82 might appropriately be called "the Frankfurter years," since at least three significant works on Felix Frankfurter were published during that period—Bruce Allen Murphy's *The Brandeis-Frankfurter Connection* (1982), Harry N. Hirsch's *The Enigma of Felix Frankfurter* (1981), and the subject of this review, Michael Parrish's *Felix Frankfurter and His Times*. Unlike Murphy's book, which focuses on the extrajudicial activities of Louis D. Brandeis and Frankfurter, or Hirsch's volume, which is a psychological analysis of Frankfurter's personality, Parrish's work is the beginning of what presumably will be a full-scale biography of Frankfurter. And in this first volume covering the precourt years of Frankfurter's life, Parrish has made a promising beginning indeed.

From his birth in Vienna in 1882 and his arrival in the United States in steerage in 1894, Frankfurter's rise to a position of power and influence in American public life is traced by Parrish in this

meticulously researched and well-written volume. Although the tough Irish youths of the Lower East Side of New York never bothered to beat him up because he was so small, Frankfurter's stature as a mature man, as Parrish amply demonstrates, was such that few of the major public problems and issues of the first half of the twentieth century escaped either his attention or influence.

Using his magnetic personality to the utmost, Frankfurter developed an uncanny ability to be befriended by a procession of the most influential figures in public life. After his graduation from Harvard Law School in 1906, Frankfurter's rise to public prominence was thus due not only to his abilities but also to his skill in assiduously cultivating the friendship of such men as Henry L. Stimson, Louis D. Brandeis, and Franklin D. Roosevelt. With their support, he progressed from service as an assistant U.S. attorney and War Department troubleshooter under Stimson, to labor mediator during World War I in the Wilson administration, to Harvard Law School professor, and before his appointment to the Supreme Court, to influential adviser to President Franklin Roosevelt and key recruiter of New Deal personnel.

Although his rise to influence was aided by a succession of important figures in public life, along the way Frankfurter emerged as a leader of the forces of progressivism in his own right, establishing a rather controversial reputation through his unflinching confrontation of some of the most emotionally volatile issues of his time. His investigations of the Bisbee deportations and the Mooney case and his defense of Sacco and Vanzetti thus brought him not only national attention but also the condemnation of the conservative establishment. Frankfurter nevertheless found time to become a leader of the American Zionist movement as a lieutenant of Brandeis.

Despite his reputation for involvement in controversial issues, it is one of the ironies of Frankfurter's precourt career that he was silent on the most important issue that affected the center of his concerns—Franklin Roosevelt's attempt in 1937 to pack the Supreme Court. Although he was silent on the issue, Parrish concludes that Frankfurter supported the substance of Roosevelt's plan and came close to breaking with Brandeis on the issue.

Frankfurter, Parrish points out, "played a central role in the history of modern American liberalism" (p. 2), and that role is illuminated by this volume that concludes with Frankfurter's appointment to the Supreme Court in 1939. Given the excellence of this effort, one is left with a sense of impatience for the volume to follow on Frankfurter's years on the court.

RICHARD C. CORTNER
University of Arizona

LINDA J. LEAR. *Harold L. Ickes: The Aggressive Progressive, 1874–1933*. (Modern American History.) New York: Garland. 1981. Pp. vii, 452. \$55.00.

In her richly documented study of the pre-New Deal career of everyone's favorite "curmudgeon," Linda J. Lear pursues two interrelated goals. The first is to analyze those personal qualities, historical forces, and crucial events that shaped Harold L. Ickes's journey from mugwump Republican to New Dealer. The second is to use Ickes's career as a "uniquely important way of evaluating the conclusions of several decades of historical scholarship against the experiences of an exceptional practitioner" (p. ii). In the main, the author performs the first task admirably while adding relatively little to our understanding of the broader questions of interpretation.

In numerous unsuccessful Chicago reform campaigns, Ickes formulated a conviction in favor of honest government and against special privilege, earning a reputation for pugnacity, honesty, and independence. Settlement-house activists Jane Addams and Raymond Robins humanized his outlook by stressing the need for government promotion of social justice, while friendships with Democratic bosses Roger Sullivan and George Brennan brought home the lessons of pragmatic, constituent-oriented, organization politics. Between 1911 and 1916, Ickes was the bellwether of the Illinois Progressive party, spearheading the abortive attempt to prevent reunification with the GOP. After sixteen frustrating years of trying to keep "progressivism" alive in the party of Harding, Coolidge, and Hoover, Ickes finally switched to the Democratic side in 1932, just in time to record his first political victories and to achieve his first public office.

For Ickes, Lear demonstrates, politics was largely a morality play with a cast of heroes and villains. Deserted by his father and bearing the scars of youthful urban poverty, Ickes constantly searched for champions of the underprivileged, finding them in John Harlan, Charles E. Merriam, Theodore Roosevelt, and Hiram Johnson. In Franklin D. Roosevelt he discovered the ultimate hero. Villains aplenty abounded among those born to the purple or those who defended special privilege—Charles T. Yerkes, George Perkins, Medill McCormick, and Samuel Insull. Flexible and nonideological, Lear argues, Ickes was not absent of ideology, professing an erratic but sincere commitment to conservation, human rights, national security, and integrity in public office. Backing FDR helped resolve an ongoing dilemma between winning and ideological integrity. Ickes was "first and foremost a progressive who joined the Roosevelt Administration because it provided him with the opportunity to remain a progressive" (p. 402).

Unfortunately, Lear is much less successful in delineating the type of "progressive" Ickes was and how his career can shed light on the progressive-liberal continuum. Indeed the author ignores the historiographical advances of the last decade; her definition of "progressive" seems close to the status revolution thesis. Although bearing a 1981 imprint and containing a 1979 introduction, the manuscript was completed in 1974 and reflects little material published after 1971. Viewing Ickes's main concern as preserving and restoring traditional American values and institutions, the author observes that he was too middle class and conservative to back Robert La Follette in 1924. Yet, eight years later, Ickes stood out as one of the few old "progressives" to back the drastic new departures of the New Deal.

Some will criticize Garland's practice of photocopying dissertations, warts and all, while others will laud the method as an economical means of publishing worthy contributions of limited sales potential. In any case, someone should be faulted for failing to correct a number of obvious "typos." Clearly the subject—and the author—deserve a better showcase. Lear has produced the nucleus of a fine book, one that could have realized its full potential by careful editorial work and by a mastery of the "progressive" historiography of the 1970s.

JOHN D. BUENKER
University of Wisconsin,
Parkside

JO ANN OOIMAN ROBINSON. *Abraham Went Out: A Biography of A. J. Muste*. Philadelphia: Temple University Press. 1981. Pp. xvii, 341. \$22.50.

A. J. Muste was a man of singular commitment and several roles. He was variously a Calvinist and religious mystic, a clergyman and a union organizer, a pacifist and an apologist for labor violence, an educator, a revolutionary, a utopian, and a loser. Yet, whatever he was, he remains one of the more underappreciated figures in twentieth-century American public life.

Born in the Netherlands in 1885, Muste was raised among Dutch-reformed Christians in Grand Rapids, Michigan. After ministerial studies, he accepted a call to a New York City pulpit in 1909 and took an immediate interest in Social Gospel Christianity. Following the outbreak of World War I, Muste moved through prayer and study into a pacifist persuasion that cost him his clerical post and drove him into the religious-pacifist Fellowship of Reconciliation (FOR). After the war, he became a union organizer and an experiment-minded labor educator. By the early 1930s, he assumed the leadership of the country's most aggressive Trotskyist

labor faction and struggled for radical social change in strikes from Illinois to North Carolina.

In 1936, Muste underwent a religious re-conversion to Christianity that moved him to devote the remainder of his life to active pacifism. Working for the FOR, he aided conscientious objectors during World War II, and, after Hiroshima, he did battle as the country's most prominent radical pacifist, determined to use Gandhi's militant nonviolence to effect racial justice at home and the abolition of the nuclear threat in the world. Through all these struggles, Muste built a unique reputation among a range of social change advocates; he was the man who could be trusted. With this reputation, a gentle manner, and political toughness, the one-time Dutch immigrant boy emerged before his death in 1967 as one of the decisive figures in the polyglot movement against the American-Vietnamese war.

A few years before his death, Muste set down some "Sketches for an Autobiography" that the writer Nat Hentoff collected in a larger volume of *The Essays of A. J. Muste*. Also in the early 1960s, Hentoff drafted an informal biographical profile of this *Peace Agitator*. Now, Jo Ann Ooiman Robinson has produced the most cogent and comprehensive study of a man that we all should know better.

Well-researched and intelligently organized, Robinson's study seeks to retrace the long pilgrimage of a man who went out into life like the biblical Abraham, "looking for a city which existed—and yet had to be brought into existence" (p. xvii). Highly sympathetic but not uncritical, Robinson recounts the obstinacy that Muste exhibited during the controversies in the 1920s involving the Brookwood Labor College and the manipulateness that he showed as a Trotskyist labor organizer. Curiously, however, she seems quite tolerant of the two largest failings of Muste's later life: (1) his capacity to excuse repressive (and sometimes aggressive) Third World socialist regimes; and (2) his determination to energize an inclusive popular movement against U.S. policy in Vietnam, while failing to maintain a clear distinction between revolutionary nonviolent resistance and violent romanticism.

Early in the 1930s, his Trotskyist comrade (and later anti-Communist avenger) Louis Budenz liked to introduce Muste as "the 'American Lenin.'" The anarchist writer Paul Goodman called him "an Authentic Great Man." The characterizations are arguable. What is less debatable, however, is that if historians still care about the lives of prominent people they ought to reflect upon the outworking and implications of the life of this man. And, when they choose to know more about him, they could do no better than to begin with Robinson's careful study.

CHARLES DEBENEDETTI
University of Toledo

EILEEN EAGAN, *Class, Culture, and the Classroom: The Student Peace Movement of the 1930s*. (American Civilization.) Philadelphia: Temple University Press. 1981. Pp. xi, 319. \$22.50.

When, some years ago, the opening scenes of "The Way We Were" were filmed at Union College, excitement was provided not only by the presence of Barbara Streisand and Robert Redford, but also by a vintage 1937 peace rally. My students were amazed to discover that such things were not inventions of the 1960s, and some were sufficiently intrigued to explore the student movement of the 1930s in seminar papers. A similar impulse, perhaps, moved Eileen Eagan to undertake this disappointing study.

Any assessment of campus activism requires an analysis of the role and structure of higher education, and of the place of students within the society at the time. The title of the present volume promises just that, but delivers only a series of gross and naively Marxist generalizations. On the basis of little or no evidence we are told, for example, that "in the twentieth century higher education responded to the needs of capitalism for new kinds of workers" (p. 5), that "the American campus both reflected and perfected the intolerance and bigotry of American society" (p. 97), and that the "students' attack on war, racism, and antisemitism was a repudiation of the existing collegiate culture and the hegemony of the ruling class" (p. 106). Some of that may indeed be true, but no more true in the 1930s than in the preceding or following decades, when there was no perceptible student movement.

If the general framework is thus hardly persuasive, the actual account of the events is painfully incomplete. Given the scattered and often ephemeral nature of the materials available, that is understandable to a degree. It is nonetheless disappointing to find the student movement defined essentially in terms of the activities of the American Student Union (and a bit of SLID), and of activism on half a dozen campuses. That Milwaukee State Teachers College gets prominent play along with CCNY, Columbia, Harvard, Pennsylvania, and Berkeley is only a slightly saving grace.

There is also a host of smaller problems. It is surely misleading, for example, to assert that Roosevelt established the Spanish embargo "in compliance with neutrality legislation" (p. 170); far-fetched to attribute campus conservatism in the 1950s even in part to the decline of the Social Gospel (p. 241); and at least disingenuous to cite as the sole example of former student radicals coming under attack after World War II, the case of the Rosenbergs and Morton Sobel (p. 240).

The student peace movement of the 1930s, like any student movement, poses substantial problems

for the historian. But without clearly establishing the numbers, characteristics and distribution of the students involved, the influence of the movement both on and off the campus, the degree to which it existed as an entity separate from other political movements in the society, and its relationship both to the youth culture and the broader political realities of the time, it is impossible to draw conclusions that deserve serious attention.

MANFRED JONAS
Union College

DOUGLAS P. SEATON. *Catholics and Radicals: The Association of Catholic Trade Unionists and the American Labor Movement, from Depression to Cold War*. Lewisburg, Pa.: Bucknell University Press. 1981. Pp. 269. \$22.50.

In 1958 Marc Karson argued that organized Catholic opposition to socialism before World War I was a major factor in shaping the conservative policies of the American Federation of Labor. Critics correctly noted that neither Father Peter Dietz nor his Militia of Christ for Social Service ever attained much influence. In fact, the Militia remained largely a paper organization of Catholic union leaders occasionally brought together by Dietz at AFL conventions. Like Karson, Douglas P. Seaton is convinced that "Catholic influence was indeed of crucial importance in the consolidation of conservative leadership, policies and practices" (p. 10) in American unions. He sets out to show that what the Militia of Christ supposedly did to the AFL, the Association of Catholic Trade Unionists did to the CIO.

Organized by a group of young Catholic idealists in 1937, the ACTU attempted to organize and educate Catholic workers, champion unions within the church, support organizing drives, and oppose corruption and communism within the unions. Seaton shows convincingly that early in its career the ACTU's anticommunism won priority over its commitment to social justice. He traces the networks of local chapters, labor schools, and individual activists through which organized Catholic influence was felt in particular unions, arguing persuasively that this influence contributed to anticommunist sentiment and that the ACTU itself worked actively for the ouster of the Communists after World War II. Unfortunately, his dependence of ACTU sources limits the impact of his argument; he shows the existence of important union-church alliances in local communities, but only in a limited number of cases he is able to show the concrete working out of that association in the daily practice of organization. Like Karson, Seaton exaggerates the significance of the church and the ACTU, but he has provided valuable information and opened up further ques-

tions about the relationship between religious belief, church affiliation, and union practice in heavily Catholic working-class communities.

Unfortunately, too, the book is flawed by simplistic definitions of conservative and radical, one-sided interpretations of Catholic social teachings, and lack of attention to some important recent work, such as that of Neil Betten, George Flynn, and Thomas Blantz on Catholics, labor, and New Deal politics. Emphasis on conservative Catholic ideology as motivating priests involved in the labor movement obscures the pastoral and organizational imperatives that led these practical, rather hard-headed men to defend unions, endorse demands for more equitable wages and for a sharing of profits and even ownership, and, for the same reasons, to oppose Communist leadership within the unions and all efforts to promote radical redistribution of power and wealth in the country generally. Catholic leaders wanted to maintain the loyalty of their people and win respectability for the church; these goals, and their own pastoral experience, made them alert to the interests of their members and the mood of the public at large. Their social attitudes and policies grew from these considerations, not from ideology considered as something apart. Given the policies supported by the Communists, and the tactics they employed in many unions, anticommunism was not only understandable but also commendable. If in the process these Catholics lost sight of the goals of social justice they preached, they were not alone, and the explanation must be sought in a wider view of religion and society in the period than Seaton provides.

DAVID J. O'BRIEN
College of the Holy Cross

LOWELL K. DYSON. *Red Harvest: The Communist Party and American Farmers*. Lincoln: University of Nebraska Press. 1982. Pp. xii, 259. \$18.95.

In this study, Lowell K. Dyson undertakes to compress the tangled, convoluted history of Communist party efforts to penetrate left-wing American farm organizations. The work encompasses the years from the early 1920s to the early 1950s, but its primary focus is on the depression years of the 1930s. It is, in part, a story of manipulation, intrigue, and recrimination, the work of a shadowy cast of characters who labored diligently within the shifting context of Kremlin dogma, a dogma that varied from the intransigent anticapitalism of third-period communism to the sycophancy of the popular front. It is also a study of little-known Communist-dominant organizations such as the United Farmers League, the Cannery and Agricultural Workers Industrial Union, the Sharecroppers

Union, the Cooperative Central Exchange, and the United Cannery, Agricultural, Packing and Allied Workers of America, the latter organization easily remembered by its alphabetic designation, UCAAWA. But it is primarily an effort to comprehend Communist efforts to manipulate the larger, left-leaning organizations such as the Farmer-Labor party of the 1920s and the National Farmers Union, the Farm Holiday Association, and the Southern Tenant Farmers Union of the 1930s. The Communist operatives engaged in these ventures consisted of such redoubtable personages as Ella Reeve "Mother" Bloor, Harold Ware, Henry Puro, Alfred Tiala, Alfred Knutson, "Red Flag" Charles Taylor, Lem Harris, Caroline Decker, Leif Dahl, and Don Henderson. None of them are likely candidates for inclusion in the next edition of *Dictionary of American Biography*.

Dyson concentrated his efforts on the 1930s. Communist organizers were most active and most reputable in these years, and the depression, presumably, made non-Communist farm leaders more receptive to a doctrinaire Marxist message. Except for a few marginal successes in California farm strikes, the Communist operatives uniformly bungled their opportunities. Even amidst the good fellowship of the popular front period, the Communists eventually provoked the hostility of "fellow traveler" farm leaders such as Archie Wright of the Dairy Farmers Union.

It is easy to lose oneself in the organizational morass of this book. Yet one can hardly blame the author. Dyson attempts to probe deeply into a human labyrinth, and many of its occupants failed to practice full disclosure. It is not always possible to distinguish Communists, Communist sympathizers, "fellow travelers," and other leftists, and at least two Communists whose activities are chronicled in this book sought elective office as Republicans. Nevertheless, as a research effort the volume is impressive. Its literary craftsmanship leaves something to be desired. In sum, Dyson recognizes that the Communist party was essentially uninterested in the farmers, that "farm work was always the party's poor stepchild" (p. 202). The result was a red famine, not a red harvest.

KAREL D. BICHA
Marquette University

ROBERT H. KARGON. *The Rise of Robert Millikan: Portrait of a Life in American Science*. Ithaca: Cornell University Press. 1982. Pp. 205. \$22.50.

The history of science is sometimes stigmatized as a parochial discipline. The charge is far from empty: historians of science tend to narrow their audience by the way they handle the intellectual substance of

their subject. Yet this is a matter of design, not necessity, and scholarship cast in appropriate terms may gain widespread appreciation—witness Daniel Kevles's *The Physicists*. In *The Rise of Robert Millikan*, Robert Kargon seeks the same success; "The book is intended for an audience wider than scientists and historians of science," he writes in the preface (p. 11). One hopes this intent will be fulfilled, for here is an admirable piece of work. Still, it is hard to dispel doubts.

Kargon has sought to illuminate and illustrate a set of broad themes by means of a biographical inquiry. Millikan (1868–1953) provides an excellent vehicle, having been cast in a series of roles suggestive of key transformations in the American scientific community and its shifting relationship to society. He was a student at the very time the critical precepts of modern physics were being established. As a maturing scientific entrepreneur he became a mainstay of the National Research Council. Subsequently he gained honors as the first American-born recipient of a Nobel prize, as a guiding spirit behind the rise to preeminence of the California Institute of Technology, and as the second most famous scientist in America (second only to Einstein).

Given Kargon's aim, this is promising material. Yet his attention remains so strongly fixed on questions of interest chiefly to scientists and to historians of science—the specifics of Millikan's research, the immediate context in which he pursued and promoted science—that his most stimulating insights tend to get lost. For example, an exposition on Millikan's contributions to physics, although illuminating on its own terms, is cast in language so specialized that the reader who is scientifically undereducated (and, alas, most of us are) will likely be skimming just at the point where Kargon introduces an astute analysis of Millikan as a "convergent thinker," as "a conservative in the midst of a revolutionary world" (p. 74) whose research aimed to protect scientific verities from radical assault, and who thought the same way about art and politics as he did about science. Or, again, a long discussion of the academic maneuvering attendant upon the establishment of Caltech, laden as it is with names few will recognize, may leave readers less than fully alert to some remarkable observations on the political economy of American science in the 1920s, when captains of commerce and industry were willing to endow scientists such as Millikan lavishly—men who "spoke their language, and shared their assumptions about the world and its future" (p. 120).

As to "assumptions about the world," Millikan emerges as a man with some disagreeable notions, not least being an ideology that was part reactionary, part technocratic. He was no small-minded materialist, surely no Babbitt. But anyone who meets Milli-

kan for the first time on the pages of Kargon's book is not likely to come away liking him much. Obviously, Kargon has not sought to make his readers like his subject, but only to understand his scientific style, his achievements, and his character, and to perceive how his life was "a microcosm of new roles assumed by the scientist during the course of the twentieth century" (p. 21). It should also be clear that, even though Millikan spent his latter years laden with honors, neither his scientific nor his social perceptions have much relevance for our own age. They are immensely relevant, however, to an understanding of science and society in his time. And, while it seems unfortunate that Kargon's pace and emphasis sometimes render his best insights less vivid than they might be, these are nonetheless important, and his book is nonetheless deserving of a careful study.

ROBERT C. POST
Smithsonian Institution

DAVID KITE ALLISON. *New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory*. (Naval Research Laboratory Report, number 8466.) Washington: Naval Research Laboratory. 1981. Pp. xi, 228.

David Kite Allison's study of the development of radar not only explains an important aspect of the history of technology but also examines the position of research and development within the navy and chronicles the rise of the Naval Research Laboratory.

During the 1930s the Naval Research Laboratory developed a practical system for the use of radio waves in detecting distant objects. Although not the first to recount this development, Allison gives a detailed and clearly written account. In the process, he clarifies and corrects previous histories of radar. He finds, for example, that the use of pulses of radio energy instead of continuous waves did not flow automatically from the laboratory's previous work in using such waves to investigate the ionosphere, as Henry Guerlac has suggested. Instead, pulse waves came to radar through the work of the sound division of the laboratory, and only after several years' work using continuous waves for detection revealed that this method was unsatisfactory.

Allison's work also highlights the value of radar for the Naval Research Laboratory. From the time of its creation immediately after World War I, the laboratory faced two quite different conceptions of its purpose. Some in the navy wished it to concentrate on narrowly defined engineering problems inherent in adapting existing equipment to shipboard conditions. Others desired what Allison calls

"mission oriented research"—investigations less tied to immediate needs but always aimed at practical applications for the navy. Questions of who should control the laboratory were closely connected to the debate over purpose. The laboratory moved from an essentially independent status to become part of the Bureau of Engineering, only to return later to independent status.

Allison is a clear partisan of broader research and independent status. Radar, he feels, played an instrumental role in helping the laboratory attain these desired goals. Radar proved so tremendously successful that it dispelled opposition to mission-oriented research. Furthermore, it reinforced those who argued that engineers and scientists deserved a position free from the control of any of the bureaus of the navy. Its story, therefore, is an ideal way to explain the evolution of the Naval Research Laboratory.

A laboratory is, of course, composed of individuals and not an abstract entity, and Allison argues that the story of technological development should also consider the people involved. Here he is perhaps less successful than in other parts of the book. The brief biographical sketches concentrate on professional experiences and qualifications. As a result, the men all appear the same, and individual dynamics within the laboratory do not emerge. Some of the quotations from interviews suggest friction or resentment was at times present—as one would expect in any institution—but the author does not pursue the subject.

New Eyes for the Navy, on the whole, is a nicely conceived and executed monograph. It succeeds in showing that important historical questions can flow from what at first would seem to be a narrow consideration of technological development.

FREDERICK S. HARROD
U.S. Naval Academy

TRAVIS BEAL JACOBS. *America and the Winter War, 1939–1940*. (Modern American History, number 28.) New York: Garland. 1981. Pp. xvi, 265. \$35.00.

The Soviet-Finnish Winter War in 1939–1940 was a sideshow affair in relation to the impending struggle between Hitler and the Allies. Yet to Americans, as Travis Beal Jacobs points out, the war was an emotional wringer. Many Americans, the press, and congressmen were torn by conflicting emotions of sympathy toward Finland and hostility toward the Soviet Union on the one hand and fears about United States involvement in the European war on the other. In the most original part of his study, Jacobs demonstrates how Americans resolved this conflict by endorsing private aid to Finland but

hesitating on the issue of official loans or arms to Finland.

Jacob's assessment of Franklin D. Roosevelt's response to the war confirms existing scholarship on his leadership and his relationship with Secretary of State Cordell Hull. Roosevelt initially tried to help Finland, most notably through an Export-Import Bank credit and in the release of forty-four Brewster pursuit planes, but he soon became cautious, providing little leadership for Congress on aid to Finland and backing away from further arms to Helsinki. Once again Secretary Hull emerges as the paragon of caution. Hull, typically worried about isolationist enemies in Congress, battled with his arch rival Secretary of the Treasury Henry Morgenthau who pushed for aid to Finland, and successfully blocked most of Roosevelt's proposals.

Jacobs offers a thoughtful, balanced conclusion on America's response to the war. The issue of aid was largely a symbolic question since there was little time for any assistance to get to Finland. The United States, moreover, had few available arms, few plants producing what Finland needed, and even fewer officials in the army and navy or among British and French purchasing commissions who were willing to give up their orders. Jacobs agrees with Robert Dallek's defense of Roosevelt's caution but also suggests, without developing this idea sufficiently, that Roosevelt missed an opportunity to influence the larger European war by demonstrating America's willingness to aid the victims of aggression.

Jacobs's book, a revision of his 1971 dissertation, tells a familiar story. But Jacobs has incorporated his earlier work in the diaries of Adolf A. Berle and Dallek's recent study in this volume in the Garland series. *America and the Winter War* not only provides a good introduction to the diplomacy of the war and the war itself but also should enlighten diplomatic historians about the American reaction and Roosevelt's response.

THOMAS R. MADDUX
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RONALD LEWIN. *The American Magic: Codes, Ciphers, and the Defeat of Japan*. New York: Farrar, Straus, Giroux. 1982. Pp. xv, 332. \$14.95.

This is another entry in the growing library of books about code-breaking in World War II. In this book veteran British military historian Ronald Lewin endeavors to make up for the lack of attention previously accorded American success in breaking Japanese diplomatic code. *The American Magic* surveys the full history of U.S. code-breaking from 1921 to 1945 including detailed information on Magic, Ultra, traffic analysis, and direction finding. One of

the more interesting aspects of the story is to see how U.S. experts moved haltingly from almost amateur cryptanalysts to full-blown experts who were reading with facility diplomatic, naval, merchant marine, and army ciphers. As the author generously admits, this story parallels and complements the tale of how the Americans matured, in a relatively short time, into a truly professional military force. In the course of detailing this story Lewin stops short of claiming too much for code-breaking, pointing out repeatedly its limitations.

Relying as it does on much recently declassified material, this book is a welcome addition to the history of the war. For, while the focus is generally on the Pacific theater, it quickly becomes evident that by reading Japanese signals we gained considerable information on other areas and participants, including the Germans and the Russians. In one of the most fascinating chapters, we eavesdrop as the Japanese attempt to convince the Russians to act as peace intermediaries in 1945. It was, as one intelligence analyst said, like observing a spaniel in the presence of mastiffs. And, amazingly, we find that the Japanese, just like the Germans, worried from time to time that the codes had been compromised—and then arrogantly dismissed the thought.

Unfortunately, there are some flaws in this generally valuable study. Although basically chronological, at times the narrative skips forward or doubles back thus making the story more difficult to follow. And the casual style, which includes many offhand comments, lengthy quotes, and possible overstatements, detracts from the credibility of this important study. Unfortunately, the paucity of footnotes will make the book less useful for historians who wish to follow in Lewin's footsteps; however, these negative factors do not outweigh the value of this welcome contribution.

JAMES R. LEUTZE
University of North Carolina,
Chapel Hill

LEON MARTEL. *Lend-Lease, Loans, and the Coming of the Cold War: A Study of the Implementation of Foreign Policy*. (Westview Special Studies in International Relations.) Boulder, Colo.: Westview Press. 1979. Pp. xix, 304. \$32.00.

Leon Martel's *Lend-Lease, Loans, and the Coming of the Cold War*, a volume published in the "Westview Special Studies in International Relations," is a case study of what political scientists like to call the "analysis gap," the gap between foreign policy intentions and outcomes. The means for accomplishing the author's purpose is an examination of the formation and implementation of four well-known diplomatic episodes often associated with the com-

ing of the Cold War: President Harry S. Truman's order to cut back lend-lease to the Soviet Union after Nazi Germany's surrender in May 1945; the lend-lease cutoff at the end of hostilities with Japan; and the delayed response of Washington to two Soviet requests for large-scale credits for postwar reconstruction. Within this period and within the space of 304 pages and 7 well-organized chapters—the remainder of the work turned over to extended notes, a modest bibliography, and an ample index—Martel, an executive vice-president of the Hudson Institute and director of its Research Management Council, has produced a judicious contribution both to the Cold War literature and the arcane world of foreign policy formation.

The author's subject matter is, of course, well known to scholars familiar with the major studies of George C. Herring, Jr., (*Aid to Russia 1941–1946*) and Thomas G. Paterson (*Soviet-American Confrontation*). What is original here is the close attention given to the implementation phase of the major decisions in question. Martel does not doubt that the decisions to end aid to Russia probably contributed to accelerating and widening the course of the Cold War and that a number of opportunities to moderate Stalin's behavior in, say, Eastern Europe were squandered needlessly. It is also conceded that Washington's decision on the credit requests, combined with the halting of reparations deliveries, probably persuaded the Soviet Union that it would have to go it alone and in so doing would have to make special efforts to maintain a large defense establishment. In a manner that challenges the views of both orthodox and revisionist scholars on the origins of the Cold War who have tended to see American decision making as generally consistent outcomes of clearly expressed preferences, Martel argues that the results were not necessarily the intention of the nation's leaders. For example, the determination to cut back lend-lease to the USSR in May 1945, the brain child of Ambassador Averell Harriman, was merely meant as a signal to the Kremlin indicating a change in attitude and not as a measure to force concessions on Poland or anything else. That such a subtle distinction should elude lower-level bureaucrats left with implementing, in the words of the order, an "immediate cutoff as far as physically practicable of other lend-lease supplies programmed for the USSR and their diversion to programs for Western Europe," should not have come as too great a surprise, but apparently it did. Steps were then taken to proceed with the delivery of goods on the high seas and on ships that had already been loaded. Moreover, given the special status of lend-lease to Russia—no information requested, no conditions of any kind—the abrupt end of the program in August 1945 should not have taken the Soviets unawares, though the manner in

which it was accomplished must have bruised some pride. Once again, Martel finds nothing in the record to support the view that the United States cut off lend-lease to Russia to apply pressure deliberately or that the brief restoration of the same flowed from any acquiescence.

In this, and in other ways, *Lend-Lease, Loans, and the Coming of the Cold War* adds a new dimension to our understanding of an important subject. To learn that such episodes were "less the product of deliberate design and more the residual of numerous attitudes and goals, each held with varying degrees of tenacity and each advocated with varying degrees of force" (p. 227) is a significant respite from the drumbeat of determinists everywhere.

JOSEPH M. SIRACUSA
University of Queensland,
Australia

ROBERT L. MESSER. *The End of an Alliance: James F. Byrnes, Roosevelt, Truman, and the Origins of the Cold War*. Chapel Hill: University of North Carolina Press. 1982. Pp. viii, 292. \$19.95.

Through his study of the bittersweet relationship between James F. Byrnes—the powerful U.S. senator, Supreme Court justice, "assistant President," and secretary of state—and Presidents Franklin Roosevelt and Harry Truman, Robert L. Messer seeks to illuminate the origins of the Cold War. Indeed, he argues that this interpersonal triangle played a crucial role in the Soviet-American confrontation of 1945–46, when Byrnes presided over the State Department. "This book reveals," Messer contends, "how human frailties can influence what is too often portrayed as a totally rational, systemic, or Machiavellian process" (p. 10).

Beginning with a discussion of the intricate maneuverings behind the struggle for the 1944 Democratic vice-presidential nomination, the author shows how Byrnes gained an undeserved reputation as an expert on American diplomacy, particularly the Yalta accords. This reputation, however, served Byrnes and the nation poorly, for Roosevelt had neglected to educate either as to the realities of wartime power politics. As a result, Byrnes followed an inconsistent course—first pursuing atomic diplomacy at Potsdam and London, later reverting to Rooseveltian conciliation at Moscow, and finally adopting the hard line of the cold warriors, among them Truman, during subsequent diplomatic ventures.

In assembling his case, the author exhibits a number of his own inconsistencies. Generally, he has made skillful use of key manuscript collections, especially the Byrnes papers that, he states bluntly, have been subject to numerous alterations and

deletions. Yet, curiously, he has neglected the massive State Department archives. Stylistically, the work is readable enough, although the author too often careens from past to present within the same paragraph. Messer is at his best in his grasp of detail. This falls somewhat short, though, of his assumed omniscience. How, one wonders, did he determine what was on Soviet Foreign Minister V. M. Molotov's mind as he joked with American diplomats?

By far the greatest irony of the book is the fact that, through his reconstruction of a complicated web of personal intrigue, the author illustrates the limitations of individual initiative in the face of global conflict. Rather conclusively he shows that Soviet wartime gains, combined with the unwillingness of most Americans to accept them, boxed in even an adroit political operator like Byrnes. Admittedly, as the author insists, "people make history" (p. 10). But this truism does not take us very far toward understanding political realities. Symptomatically, Messer writes that Byrnes, despite his pursuit of the vice-presidential nomination, "inexplicably" refused to talk with the CIO's Sidney Hillman (p. 27). But was it really surprising that a Southern reactionary would avoid playing up to a leftist Jewish labor leader? Politics, after all, is also about competing ideals and interests.

This book, then, clarifies many personal details. But one puts it down wondering if, ultimately, they are not peripheral to issues of global power and politics.

LAWRENCE S. WITTNER
State University of New York,
Albany

THOMAS A. BRYSON. *Seeds of Mideast Crisis: The United States Diplomatic Role in the Middle East during World War II*. Jefferson, N.C.: McFarland. 1981. Pp. viii, 216. \$16.95.

The era of World War II, which marked the coming of age of American foreign policy in general, was also the time of the first serious and prolonged involvement of U.S. diplomacy in the Middle East. Given the availability of archival sources and the growing interest in the subject, such topics as the origins of Cold War conflict in the region, the development of American-Saudi relations regarding oil, and the background of the Palestine question have all inspired specific studies in recent years. Thomas A. Bryson gives a brief overview of these and other developments. He surveys American relations with the different countries in the area, the economic policies of the Anglo-American Middle East Supply Center, the diplomacy behind the 1942 invasion of North Africa, the growing importance of Middle Eastern oil in the eyes of U.S. policy

makers, and Washington's tendency to oppose the reimposition of French colonialism in the region.

The book's strength is also its weakness. As an introduction to the major issues of American-Middle Eastern relations in the period, it is often concise and readable, but the book is heavily narrative and skimpy on analysis. Many of the debates and new information assembled in recent years are not found here, perhaps due partly to Bryson's untimely death. His dependence on the *Foreign Relations* volumes, rather than use of the archives themselves, and the fact that the story ends with the conclusion of the war, thus omitting the important crises of 1946-48, are also problems.

When Bryson does try larger interpretations, they are generally reasonable ones. He notes that earlier in the war the United States tended to follow Britain's lead and that, even later, Washington competed with London more for economic opportunity than for political influence in the region. Always and everywhere, the main American motivation was to win the war in the most efficient manner. This also explains the course of American-Soviet relations in the region. Roosevelt wanted to avoid conflict with Moscow both for the sake of the war effort and to maintain the prospects for postwar cooperation.

There are a number of other important factors that are only briefly dealt with in the book. These include the local perceptions of U.S. policy and attempts to affect it, the British side of the story, the growing mistrust of the Soviets and the frictions with them over Iran and Turkey, the thinking of various individuals within the U.S. policy-making apparatus, and the mix of forces affecting American policy over oil and Palestine.

Of course, this is a big order for any one, small book. For those interested in understanding the issues that shaped U.S. policy in the region and American behavior there, Bryson has produced a brief and useful volume.

BARRY RUBIN
Georgetown University

LAWRENCE S. WITTNER. *American Intervention in Greece, 1943-1949*. (Contemporary American History Series.) New York: Columbia University Press. 1982. Pp. xii, 445. \$19.95.

The study of American policy in Greece in the aftermath of World War II contributes much to the understanding of the origins and tenor of the Cold War. In particular, it illustrates Washington's tendency to view instability and political violence abroad exclusively in terms of the East-West confrontation and to regard militant leftists as pliant tools of Moscow. Determined to suppress such

adversaries, the United States, while professing to serve the cause of democracy, was prepared to support repressive regimes, to intervene massively in the domestic affairs of client states, to insist on military solutions to fundamentally political problems, and generally to harness the interests of other peoples to American interests. In addition, Washington's perception of the Greek crisis reveals a mixture of inflated confidence and naive optimism that was to be the hallmark of much American foreign policy until the 1970s. In the words of one American official quoted in this book, there was "nothing wrong with Greece that time, forceful U.S. guidance and American dollars will not correct" (p. 226). Thus the success of its intervention in postwar Greece may have encouraged the United States to intervene in other regions, including Southeast Asia.

This is clearly the central theme of Lawrence S. Wittner's meticulously researched and well-written *American Intervention in Greece, 1943-1949*. Utilizing recently released documents and a plethora of other published and unpublished materials, Wittner traces with skill and precision the formulation and implementation of American policy from the war years, when Britain was dominant in the Greek political arena, until the defeat of the insurgent "Democratic Army" in 1949. In a concluding chapter he surveys Greek-American relations since the 1950s, blaming the legacy of the Truman Doctrine's ideological rigidity and high-handed tactics for Washington's support of the Greek junta and for the strain between the two governments since the junta's fall in 1974. His overall assessment of what he has set out to investigate is stated in his preface, where he asserts that his findings generally support the "revisionist" side in the debate over the causes of the Cold War and that his is a "startling, even jarring, account of U.S. government behavior" (p. xi).

Wittner argues that during the war and despite occasional disagreement over tactics, the United States generally supported British schemes to suppress the Greek left (the dominant element in the wartime resistance movement) and to restore the unpopular king, whom London viewed as the best guarantee that Greece would remain outside the Soviet orbit. After 1944, while still reluctant to become directly involved in the morass of Greek politics, Washington took an increasingly active part in Greek affairs, motivated by a growing fear of Soviet expansionism in the direction of the eastern Mediterranean and the Middle East. In 1947 the Truman Doctrine signaled a deliberate decision to consider the defeat of the Greek insurgents as a requirement of the strategy to contain the Soviet Union. This step soon led to American control of virtually every aspect of Greek affairs, extending

from political, economic, and military matters to those pertaining to trade unions, internal security, detention camps, and death sentences. At the height of the crisis serious consideration was given to dispatching American combat troops to help defeat the insurgents. In addition to narrow security considerations, concern for oil and for (vaguely defined) business opportunities abroad, as well as a determination to expand American influence and power generally, helped shape the Truman administration's position in Greece. Consistent with this outlook, the United Nations was treated alternatively as a tool of, or impediment to, American policy.

A key point in Wittner's account, and one that is now almost universally accepted, is that the Soviet Union did not provoke the Greek insurgency and may in fact have worked behind the scenes to bring about its end. The communist-controlled uprising is shown to have been initially defensive in character, sparked by persecution and terror at the hands of the rightist "parastate" if not of the government itself, and fueled by economic collapse and governmental inefficiency and corruption. After 1948 the insurgents' only patron, Yugoslavia, helped seal their fate: following the Stalin-Tito break (in which the dominant faction of the Greek Communist party sided with Moscow) the Yugoslavs closed their border, thus facilitating the victory of the Greek government forces in 1949. Although the Yugoslavs and Greeks on whose writings he often relies had their own axes to grind, Wittner's treatment of the insurrection is basically sound and well documented: this was a civil war in which the antigovernment forces received little outside encouragement and less material assistance. His portrayal of the Greek communists is rather idealized, while that of their opponents is too sweeping and harsh; nevertheless, he offers considerable evidence to support his characterizations. Several of his conclusions, however, are bound to be challenged. In particular, the assertions that in strictly military terms the role of the United States in the defeat of the insurgents was less than decisive (a point made by several British writers), and that the long-term effects of American economic aid may not have been beneficial to the Greek people, need to be argued much more fully if they are to be convincing. Similarly, the suggestion that American intervention added to the oppressive character of the Greek regime and to the brutality of the civil war is hardly substantiated by the evidence provided here.

Wittner is at his best when he pieces together the many elements of American policy toward Greece from inception to implementation. He is particularly effective in showing that American perceptions of the problems besetting postwar Greece were distorted by Cold War fixations. He is less persuasive when he speculates as to what would have happened to

Greece had there been no American intervention and no suppression of the communists. The argument that under broadly based coalitions Greece would probably have emulated a Titoist brand of socialism and nonalignment rests on a highly subjective assessment of the forces at play inside and outside Greece in the 1940s and 1950s. It also holds precious little attraction among Greeks, no matter how bitterly they may resent the role of the United States in their country.

One need not agree with all its conclusions to welcome this addition to the systematic study of the Cold War. Wittner has written an important book, rich in factual material and in argument.

JOHN O. IATRIDES

Southern Connecticut State College

R. ALTON LEE. *Dwight D. Eisenhower: Soldier and Statesman*. Chicago: Nelson-Hall. 1981. Pp. xii, 379. Cloth \$21.95, paper \$10.95.

R. Alton Lee concludes this judicious, compact biography by noting that "historians are coming full circle by evaluating Eisenhower as favorably as did his contemporary American public" (p. 336). By the time this judgment is reached, enough detail and analysis have been presented to make this a sober assessment of Eisenhower's remarkable career. In effect, Lee has accomplished a smooth, useful synthesis of more than a decade of scholarship, and it is likely to remain as a handy summary of what has been written until now.

He has furnished both an introduction to the Eisenhower literature and also an evaluation that specialists can use. His review of the general's military career accepts Stephen E. Ambrose's acknowledgement of the achievements of a man Kenneth S. Davis long ago called the "soldier of democracy." Lee agrees that Eisenhower was "a great tactician in his own right," and, in the field as well as in staff headquarters, "events usually proved him correct." Together with General George C. Marshall, "this fundamentally peaceable man" deserves the credit for the Allied success in World War II (p. 116). Once the war was out of the way, Eisenhower explained that his major concerns were the need for universal training, unification of the military services, and international cooperation.

Subsequent events, especially during his presidency, brought a remarkable dedication to his concepts of universalism, more pejoratively known as "globalism." Having achieved his initial political goal of fending off the more extreme nationalists from control of the Republican party, Eisenhower the "cold warrior" was also very much in charge as was Eisenhower the conciliator. Lee argues, along with other recent scholars, that John Foster Dulles

"served the same function in foreign policy that [Sherman] Adams served in domestic affairs or Richard Nixon did in the political arena—he was unpopular and thus served as a lightning rod to draw the bolts away from the president" (p. 184). Lee inevitably repeats the familiar reservations about Eisenhower's responses to Joe McCarthy and the civil rights movement, and, as with everything else in this biography, these issues are handled coolly—perhaps too dispassionately for some readers. At no point, however, does Lee betray his sense of historical perspective.

Throughout this phase of Eisenhower historiography, it has been hard to separate analyses from contemporary influences. The relationships have often seemed obvious. Journalists began to take another look while American policies in Southeast Asia were deteriorating. When historians began their own explorations, they did so against the background of an increasingly contentious era. By the time more relevant files from the Eisenhower library became available in the 1970s, it had become even easier to view the Eisenhower presidency as the most successful since World War II. Lee believes that this will continue, and that additional findings will "probably demonstrate that he [Eisenhower] accomplished a great deal more in those eight years of his presidency than has hitherto been believed" (p. 335). It should be interesting to see what the course of such scholarship will tell us about our own times. Whatever direction may be followed, Lee's book will have served its purpose.

HERBERT S. PARMET

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LEONARD DINNERSTEIN. *America and the Survivors of the Holocaust*. (Contemporary American History Series.) New York: Columbia University Press. 1982. Pp. xiv, 409. \$19.95.

The meaning of America has been the subject of debate since colonial times. Freedom, opportunity, and tolerance have been praised as hallmarks of the New World—a beacon to millions of oppressed from the Old. Yet, image and reality were often poles apart. And eventually, as massive foreign labor was no longer required, the doors were shut with racial or ethnic restrictions.

In this scholarly contribution to the history of immigration and ethnicity, Leonard Dinnerstein has written more than a specialized work. He presents an appraisal of America at mid-twentieth century that challenges common perceptions.

That America failed to offer a haven to refugees from Nazi persecution and genocide is known. A

combination of biased immigration law and prejudiced officials, the challenges of depression and war, chronic antisemitism, and public indifference contributed to the Holocaust's tragic dimensions. Remarkably, as Dinnerstein reveals, not much changed with the coming of peace. Whatever the outcome in power politics, there was no clear victory of compassion over racism.

Of those who survived the concentration camps, one-third died within a week of liberation. But the others languished in assembly centers lacking even minimal facilities or medical care, often herded together with non-Jewish displaced persons (DPs) who were Nazi collaborators. By an immense irony, German Jewish survivors were denied ration cards as "former enemies," while Ukrainian collaborators received favorable treatment as "Allied" nationals and anti-Communists.

Dinnerstein weaves an astounding yet well-documented narrative of human suffering and the callous response by occupation commanders, bureaucrats, politicians, and occasionally blatant antisemites. Among the latter was General George S. Patton, who wrote in his diary that DPs were less than human but Jews especially "lower than animals" (p. 17).

Actually, at the end of 1945 Jews constituted less than 10 percent of the DPs (though all too visible to hostile observers) within a human sea of Balt, Slavic, and "ethnic German" nationalities. Altogether over a million people were stateless wards of the Western Allied armies, aided ineffectually by the UN Relief and Rehabilitation Administration (UNRRA) as well as voluntary social service agencies.

The Truman administration's investigating commission exposed the deplorable conditions in refugee camps. Clearly, however, resettlement was the most promising course, affirmed in a unanimous recommendation of the Anglo-American Committee of Inquiry that displaced Jews be admitted to Palestine. But British colonial policy kept Palestine's doors shut as they had been throughout the Holocaust era.

In America, lobbying by humanitarian groups and President Truman resulted only in a flawed and biased Displaced Persons Act of 1948. It effectively excluded most Jews by means of a cutoff date *prior* to the release of Jewish survivors from Russia and of refugees from postwar pogroms in Poland, as well as a lopsided quota favoring nationals of "annexed" countries and farmers.

Anti-Jewish features inserted by senators preferring "good blood" (p. 169) were not modified until 1950. Even then, Senator Pat McCarran (R., Nevada) and others exploited administrative pressures to achieve their goal of keeping down the number of Jews. As Dinnerstein concludes, U.S. immigration policy "failed to meet the needs of the majority of the Jewish D.P.'s" (p. 271).

Throughout, Dinnerstein provides incisive analyses of public opinion (including the Jewish community) and sociopolitical crosscurrents. His conclusions are buttressed by meaningful statistical data and useful appendices.

JOSEPH BRANDES

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HARRY R. BOROWSKI. *A Hollow Threat: Strategic Air Power and Containment before Korea*. (Contributions in Military History, number 25.) Westport, Conn.: Greenwood Press. 1982. Pp. xii, 242. \$27.50.

With emphasis on the Strategic Air Command (SAC), Harry R. Borowski examines the military capability of the United States between the end of World War II and the beginning of the Korean War. This period saw the rapid demobilization of the vast army, navy, and air force that had won the war; the agonizing process of reorganization (unification) of the armed forces; the formulation of the Marshall Plan and the policy of containment; the Berlin blockade and airlift; the fall of Nationalist China; Communist takeovers in Hungary and Czechoslovakia; the NATO treaty; the Russian explosion of an atomic device; and Truman's decision to go forward with the development of the hydrogen bomb. In short, it was a transitional period that bridged the chasm between wartime cooperation and the advent of the Cold War. At the beginning of this epoch the United States was attempting to banish the atomic bomb from international affairs, while at the end of the period American defense and foreign policy rested on ever more destructive bombs.

The thesis of this book is that in relying on atomic weapons the United States posed a hollow threat. The United States' monopoly of atomic bombs was neither a credible deterrent nor a viable instrument of policy for two reasons. In the first place, the supply of bombs was very limited. While officials have never disclosed the exact number of bombs available, the best guess is that there were no more than thirty bombs in mid-1947. Even if the supply had been adequate, SAC, the agency responsible for delivery, was in its early days operationally substandard and unable to respond promptly and efficiently to a crisis situation. A considerable portion of Borowski's book is devoted to the efforts of General Curtis LeMay to improve the training, morale, and general efficiency of SAC units.

The general thesis of the "hollow threat" is not new; it has been suggested in several books and articles. Compared to previous accounts, however, Borowski develops the theme in greater detail, with more specific and thorough references to official Air Force documents. His book will be of value to students of national-security and foreign policy,

especially those interested in the origins of the Cold War, but it will have less appeal to the general reader because it fails adequately to relate the history of SAC to the larger national and international picture. The bibliography of original air force sources is excellent; the style is pedestrian; and the general format of the book, apparently typewriter composed with a ragged right margin, is not especially attractive.

HARRY L. COLES
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RONALD W. PREUSSEN. *John Foster Dulles: The Road to Power*. New York: Free Press or Collier Macmillan, London. 1982. Pp. xiv, 575. \$19.95.

This is the first of a two-volume work that promises to be the definitive biography of John Foster Dulles. It takes the reader through 1952 when Dulles was about to realize his ambition of becoming secretary of state. Ronald W. Preussen's work, a masterpiece of research and biographical analysis, presents an account of Dulles's personal and intellectual development as a prelude to his later performance as secretary of state.

By analyzing the biographical material for different periods in Dulles's career, Preussen shows how certain core values and perspectives were acquired that defined reality and circumscribed Dulles's view of international politics. Dulles's family and education at Princeton, his work as a counsel to the Allied reparations commission at the Paris Peace Conference, his involvement as legal counsel to innumerable banks and corporations involved in foreign investment and financial operations, the chastening experience of the Great Depression, his reaction to the rise of the dictators and the onset of World War II, and finally his wartime leadership of the Protestant Churches Commission for a Just and Durable Peace were all reflected significantly in his intellectual experience and development.

Preussen convinces us that each experience added to Dulles's intellectual and political background, broadening his range of knowledge and the conceptual scope of his thinking. In this way he gained the stature and reputation essential to his subsequent advancement. Preussen also demonstrates in convincing and meticulous fashion that none of these experiences really altered the core values of Dulles's operational code.

What were the core values and perspectives that Preussen finds ultimately informed and limited Dulles's political code and behavior? An innate conservatism, a fear of revolution and disorder, an almost panacealike faith in the prophylactic value of free trade and a stable international monetary system as an antidote to war and conflict; a belief that tension and war were caused by the conflict between

rising and static powers and that if war was to be avoided changes in territory, wealth, and influence ought to be accepted but not changes that threatened the existing order; a belief in American exceptionalism, and a born-again commitment to Christianity as the ultimate vindicator of America's (and of his own) righteousness. This is not an unfamiliar listing of the traits we identify with Dulles, but Preussen's research and analysis give them such an overwhelming substantiation as to be utterly convincing to anyone seeking to understand Dulles's character and political behavior.

Preussen devotes the second half of this volume to Dulles's performance first as foreign policy adviser to the unsuccessful Republican candidate, Thomas Dewey, and then as adviser and bipartisan participant in the formulation of policy under Marshall and Acheson. Here the value and power of Preussen's work for illuminating and explaining Dulles's advice and behavior in the initial Cold War period quickly becomes apparent.

The traits and behavior that we identify as the hallmark of John Foster Dulles, the statesman, emerge clearly and convincingly with all their significance for American diplomacy thanks to the intellectual effort that has gone into this splendid biography. Preussen has given us a powerful demonstration of the subtle ways in which the values and interests of a ruling class and civilization, working through a particular individual, influence and determine the perceptions and definition of a country's national interest. An important work.

DAVID S. MCLELLAN
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MICHAEL O'BRIEN. *McCarthy and McCarthyism in Wisconsin*. Columbia: University of Missouri Press. 1980. Pp. ix, 269. \$20.00.

This is an expansion of Michael O'Brien's excellent 1971 doctoral dissertation at the University of Wisconsin, Madison. It is an effort to describe in detail Joe McCarthy's history within his home state and to gauge the impact of his anti-Communist exploits on Wisconsinites. The study does not shed new light upon Senator McCarthy's anti-Communist rampages in Washington, nor does it pretend to.

The book contains several notable features, including a useful biography of McCarthy's early years, solid accounts of the election campaigns of 1946 and 1952, and a careful examination (previously published) of the senator's reckless attack in 1949 upon the liberal *Madison Capital Times*. Particularly interesting is the well-documented account of Wisconsin's extraordinary resistance to the second Red Scare. Despite considerable support for McCarthy within the state, "Wisconsin's institutions defeat-

ed action that threatened civil liberties" (p. 192). The story of the University of Wisconsin's subtle and successful battles to preserve its integrity from right-wing attacks makes fascinating reading.

The author, an associate professor of history at the University of Wisconsin Center, Fox Valley, deserves special praise for more than a decade of diligent research on his topic. No student of McCarthy has worked more extensively in state newspapers or has a firmer grasp of relevant archival materials. Moreover, O'Brien conducted 102 interviews, several of extraordinary value. His "Essays on Sources" is one of the best in print on McCarthy and his "ism."

Unfortunately, this study also contains a number of errors, omissions, and misinterpretations. O'Brien relied too often on journalistic accounts by Richard Rovere and Jack Anderson, for example, and this compelled him to accept at face value a number of false charges leveled by McCarthy's enemies, such as the "quickie-divorce scandal" of 1946 and later allegations involving the sugar and housing industries. He exaggerates the significance of McCarthy's 1936 race for district attorney, rejects the accurate account of how McCarthy financed his election race of 1939, calls Jean Kerr McCarthy's "secretary" (p. 177), and thinks that the conclusion of the Korean War marked the end of the second Red Scare. The most serious problem is that despite his many years at work on McCarthy, O'Brien has only a rather superficial understanding of this complex and tragic man. It is wholly inadequate to dismiss McCarthy simply as "boisterous, impetuous, and cynical" (p. 126). A cynical man, as I have pointed out elsewhere, could easily have deflected the blows of 1954 and continued as a major figure within Wisconsin and the nation.

This book is stronger than its sometimes glaring weaknesses, however, and should receive more attention than it has to date. On the whole, it is a solid contribution to Wisconsin history and to our knowledge of recent politics.

THOMAS C. REEVES
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BARBARA BARKSDALE CLOWSE. *Brainpower for the Cold War: The Sputnik Crisis and the National Defense Education Act of 1958*. (Contributions to the Study of Education, number 3.) Westport, Conn.: Greenwood Press. 1981. Pp. x, 225. \$27.50.

In 1961 a departing president of the United States, Dwight Eisenhower, warned against the dangers to American democracy stemming from the rise of what he termed "the military-industrial complex." Barbara Barksdale Clowse's study of the impact of

Russia's Sputnik in precipitating the passage in 1958 of the National Defense Education Act might be expected to shed some light on the educational involvement of a society in which Cold War thinking predominated and the military-industrial complex exerted enormous influence. Unfortunately her detailed itemization of the legislative developments that led to the NDEA raises more questions than it answers.

The book gives us a clear, straightforward account of the genesis of the 1958 education bill. We are told about the roles played by the president, cabinet members, congressional leaders, and various spokesmen for education and science. We learn about the frantic lobbying, the committee hearings, all the "wheeling and dealing" that went into the making of this act. This step-by-step legislative history is thorough, almost too much so. The problem is that the deeper meanings of what transpired are not adequately explained. For example, Clowse observes that the NDEA was passed in large part because of "the extremities of the Cold War" and quotes from sources that found this situation to be "ironic" if not downright "reprehensible" (p. 147). But having raised this question about the kind of world in which America's leaders found themselves, in Robert Hutchins's words, "assimilating education to the Cold War and calling an education bill a defense bill," she fails to explore its full implications. Instead she quickly drops the issue and proceeds to a discussion of legislative precedents for NDEA. Again, she notes in another place the difficulties Americans faced when they contemplated enacting a bill that would single out especially talented individuals for aid. "The idea of special schools for gifted students," she writes, "was drawing fire because it smacked of elitism. Apparently, an aroused and frightened populace was no guarantee that elitist programs would be accepted" (p. 74). But having raised this vital point, she once again fails to explore its implications, either in relation to America's historical development or in terms of the nation's contemporary orientation.

The author is a historian, and her book is in many ways a workmanlike production. She uses a wide variety of original sources and has surveyed much of the contemporary periodical literature on the subject. In addition, she has consulted a number of works on educational history and philosophy, though there are some significant omissions here of volumes that would have helped her elucidate the historical background more fully. The main difficulty with the book, however, is that the developments in this crucial period of American legislative and educational history are recounted in detail without a sufficient effort to probe beneath the surface and ascertain their deeper significance.

WILLIS RUDY
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CANADA

FREDERICK J. THORPE. *Remparts lointains: La politique française des travaux publics à Terre-Neuve et à l'île Royale, 1695-1758*. (Cahiers d'Histoire de l'Université d'Ottawa, number 11.) Ottawa: Éditions de l'Université d'Ottawa. 1980. Pp. 194. \$6.75.

This is an administrative history of the financing and construction of fortifications at Placentia in Newfoundland and, after 1713, at its successor, Louisbourg. Since Louisbourg was twice captured and the building of the defenses was interminable and costly, one might suspect that it was a wasted effort. Frederick J. Thorpe rejects this pragmatic evaluation because the objectives of the French crown were economic and not strategic. The fortified ports were to protect French vessels engaged in the seasonal cod fishery off Newfoundland; they were not offensive bases, as the English believed.

The French government's concern for the cod fishery was combined with private economic interests. The Louisbourg project is described as a "trans-Atlantic extension of the 'French fortress industry'" that existed to maintain the border defenses of France (p. 167). This "industry" linked royal officials, military engineers, builder-contractors, and suppliers in mutually beneficial relationships. As a result of this transplanted industry, the conservatism of the corps of military engineers, and the expectation that the work would be swiftly completed, the French failed to adapt the fortifications to the climate and resources of Atlantic Canada. Most of the building materials, stone included, were imported. Completion of the projects was delayed by limited funds, changing priorities, personal conflicts, speculation, and a shortage of craftsmen and trusted materials.

The building and manning of fortifications on Newfoundland and Cape Breton were France's greatest financial contribution to these French possessions. The government accepted the cost and prolongation of construction because "the interminable building programs were . . . an important form of economic activity, not only because they served metropolitan interests, but also because they were profitable to the trade of Ile Royale [Cape Breton]" (p. 55). Since the local population also depended on these military expenditures, they were a boon to the colonial economy that could not be abruptly terminated. Thorpe argues that the defenses did not fail the test of war for in 1758 Louisbourg "fulfilled . . . the classic role of fortresses: that of attracting a more numerous enemy and of delaying the execution of his plans" (p. 168).

Thorpe's principal source was the correspondence of French colonial officials. The administrators' preoccupations have shaped the book. The measures used to compel shipowners to carry build-

ing lime or workers to Placentia, terms of contracts, and methods of accounting are recounted in detail. Although there is a glossary of the terms of military architecture, there is no discussion of the value of the fortifications as defenses. The illustrations, which might have been informative in this regard, are often of poor quality; they lack critical captions and are not coordinated with the text.

Remparts lointains presents the fortifications as beneficial make-work projects executed by competent and persevering individuals despite many handicaps. Only in the conclusion does the author reveal some shocking flaws in the work, such as the use of salty sand for mortar or location of a battery below an undefended hill. These blunders are not explained. An administrative history with a cost-benefit analysis may open new perspectives on defense expenditures, but it does not tell the whole story.

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PAUL CRAVEN. *"An Impartial Umpire": Industrial Relations and the Canadian State, 1900-1911*. (The State and Economic Life, number 3.) Buffalo, N.Y.: University of Toronto Press. 1980. Pp. x, 386. \$13.50.

This volume, based on Paul Craven's dissertation in sociology at the University of Toronto, appears in "The State and Economic Life" series, edited by economist Mel Watkins and political scientist Leo Panitch. Thoroughly interdisciplinary, it is a good example of both the renewal of political economy in Canada—a tradition founded by scholars such as Harold Innis but which went into precipitous decline in the 1950s and 1960s as political science and economics fled in different directions—and an emerging school of Marxist scholarship with its roots in the 1960s. Although not all new Canadian political economy is Marxist, much of it, including this volume, is. Unlike some of that Marxism, however, this volume is thoroughly historical and makes a significant contribution to Canadian labor and working-class studies.

Focusing on Mackenzie King, the least likely and most studied Canadian prime minister, Craven traces the "structural, institutional, and ideological preconditions for the emergence and reception of the new industrial relations policy," (p. 9) namely the constellation of pre-World War I legislative changes that included the creation of the Department of Labour, the founding of the *Labour Gazette*, and, more importantly, the passage of various federal conciliation and mediation acts, culminating in the Industrial Disputes Investigation Act of 1907. The second part of the book analyzes that "policy in its practice" (p. 9).

The book is extremely ambitious and, though the

author is at times a bit pompous, it deserves a careful reading. The extremely detailed material on King's intellectual development at Toronto, Chicago, and Harvard recreates the deep tensions in the social sciences, which were desperately trying to decide if they were social or scientific. The author, however, is far less successful in his attempts to delineate the economic background and the class contexts in which Mackenzie King paraded his social program for industrial peace. Overly influenced by the focus of non-Marxist political economists on the staples in Canadian development, Craven downplays the much more recognizable aspects of a Canadian industrial economy undergoing rapid centralization and concentration. His self-admittedly reductionist substitution of the Trades and Labor Congress (TLC) of Canada for the working class and of the Canadian Manufacturing Association (CMA) for capital simply will not do. (He himself refers to this as "reification" [p. 10]). Although this was a decade of significant consolidation for Canadian labor and capital, regional distinctiveness of workers and bosses had not been so totally eroded.

Although undoubtedly a significant improvement over early corporate-liberal interpretations of Canadian progressivism, this volume would have been far more convincing if it had subjected Canadian workers and their employers to as detailed an empirical examination as it devotes to Mackenzie King and his accumulation of "intellectual capital," as King once described his education. Instead we are given the CMA and the TLC, a far simpler world and one that King, undoubtedly, would have much preferred.

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JONATHAN F. WAGNER, *Brothers beyond the Sea: National Socialism in Canada*. Waterloo, Canada: Wilfrid Laurier University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. xxiii, 163.

Jonathan F. Wagner has published widely on topics connected with German and German-Canadian history. This volume is equipped with a bibliography and index, and a chapter of conclusion is also provided. Its title derives from a pro-Nazi novel of 1938 that discussed the racial and cultural "extinction" of German minorities abroad, with particular reference to Canada. In Wagner's words, the objective of his present volume is "to describe the membership, the organization and the expression of the Canadian National Socialist movement, and to analyze the reasons for its being, its aims and its accomplishments" (p. xxi).

The title of the book is to some extent a misnomer because the text only deals with National Socialism

among *Germans* in Canada. Most of the "Germans" of western Canada had been for generations the citizens of countries other than Germany. In view of this, more thought could have been given to the vast differences in the backgrounds of the various groups, some of whom had never experienced the impact of a united Germany. The interest and scholarly attention paid by the *Deutsches Auslands-Institut* to the characteristics, vocational and otherwise, of the German settlers in Canada is hardly surprising. It would be wrong to suppose that only Canada was selected for such examination; all the sizable German minorities were studied with similar intensity by various Reich-sponsored agencies. Wagner found the *völkisch* movement very pervasive, particularly in western Canada, and thus he is inclined to regard the West as more important for the objectives of his investigation. Some German observers assumed German-Canadians could become what the former termed *Germanissimi Germanorum* ("more German than the Germans") because those who had come from Russia, the Banat, or Transylvania had long been the defenders of German traditions. Therefore Wagner thinks they must have grasped the deeper meaning of *Volk* unity (p. 27).

Volk—and its adjective *völkisch*—implied closeness to the land and nature (p. 15); thus greater importance was attached to the work of Germans who tilled the land. Most of these had settled in western Canada. Canadians of German descent living in eastern Canada constituted a less attractive proposition for Nazi organizers. The term *Volk* also implied the optimum level of cultural integration on the road to national unity. Proselytizers of the Nazi creed tended to look down on Canada as being an "artificial society" without "the heart of a nation." Their propaganda presented German settlers in Canada as living "in an inferior 'empty civilization.'" Some German authorities on Canada were of the opinion that its German minority should follow the example of French-Canadians in their struggle "against a hostile majority" (pp. 25–26).

After the Nazi takeover in Germany came an almost immediate worldwide organizational drive in which Canada played only a comparatively minor part. The Nazi penetration of Canada began early in 1933 through the agency of the American-run *Friends of the New Germany*. This attempt failed owing to clumsiness of approach (pp. 64–65). The setting up in 1934 of the *Deutscher Bund Canada* resulted in a more lasting organization. As in other countries, the Nazi doctrine proper was disguised under an anticommunist and antisemitic stance. Racial superiority was emphasized, as exemplified in an address to an eastern Canadian *Ortsgruppe*: "The German is martial, heroic, ideal, and social; the Jew is non-military, cowardly, materialistic, and parasitic" (p. 73).

The *Bund* was at its strongest in western Canada. Winnipeg had two *Ortsgruppen*. Saskatchewan, with its large German population, possessed *Stützpunkte* and *Ortsgruppen* in about forty cities. Moreover, Edmonton, Calgary, and Vancouver had *Ortsgruppen*, and some *Stützpunkte* were scattered through rural areas. *Bund* membership does not seem to have been more than two thousand at any stage, accounting for a very small percentage of people of German origin. What is even more interesting is that not more than 88 Nazi party members were listed in Canada in 1937—in other words, less than 5 percent of the Bundists (pp. 68–69). As to the membership, “the *Bund* attracted members who were basically petit-bourgeois in status or in inclination. . . . Besides being marginal socioeconomic types, Bundists were in the majority of cases new immigrants or at least first-generation Canadian Germans. In Saskatchewan the *Bund* generally did not develop in the pre-1900 German settlements, but in towns founded after the turn of the century” (pp. 70–71). Most members were little acquainted with the *völkisch* ideology and they tended to be less than forty-five years of age.

Wagner also calls attention to the fact that four traits of the Bundists—their “youth, their status as unassimilated immigrants, their petit-bourgeois values and their relative poverty”—accounted both for their radicalism and for their attachment to National Socialism. Then too, all this was occurring during the Great Depression. “By presenting a simplistic explanation for the economic crisis—as being due to the ‘Jewish world conspiracy’—National Socialism provided the Bundists with a convenient rationale for their economic troubles” (p. 72).

During the period of the *Bund*’s popularity seven major German-language weeklies, including the *Bund*’s propaganda organ, the *Deutsche Zeitung für Canada*, distributed reading material to subscribers, mainly in western Canada. In addition, a number of religious papers were published by the Mennonite and the Roman Catholic congregations. Five of the seven German newspapers in Canada echoed the teachings of Nazism. The various agencies of Nazi propaganda in Canada, however, did not divide their tasks clearly or support one another’s work. On the contrary, they were bitterly competitive (p. 146). For the *Volksdeutsche* (German-speaking settlers from countries other than Germany) specific points in the Nazi program had little significance: “the Nazi ravings against the Jews and the communists or the incessant calls for teaching the children the German language simply did not constitute a practical programme worthy of support. For most German-Canadians, such demands were too abstract and too unrelated to Canadian social and economic realities” (p. 146).

Early opposition to the movement came mainly from the Canadian Jewish Congress. Members of

the Congress analyzed the German press to identify the nature of its antisemitism. Speakers were appointed to refute publicly anti-Jewish slander and so on. The communists, too, called public attention to the activities of individuals and official German agencies. By the end of 1938, protests and petitions from individuals were demanding action against indigenous Nazis, and the German General Consulate found it necessary to raise objections to a district judge’s calling the Nazi leaders of Germany “the mad dogs of Europe” and to a school principal’s reference to the Führer as a “hate-crazed psychopath” (pp. 127–28). By the middle of 1939, lists of Nazi sympathizers had been supplied by the Royal Canadian Mounted Police to the federal government. Soon after the outbreak of hostilities on September 4, 1939, the leading Nazis of Canada were detained. At various stages during the war most of the Nazi detainees were released, with only “the most fanatical of Hitler’s supporters” left in confinement.

In sum, the likely candidates for the pro-Nazi movement in Canada were people who, owing to their youth or lack of stable “anchor points” in their personalities, or because of the Depression or their newness to the country, were still strangers to Canadian society. The very cultural homogeneity among pro-Nazis militated against the movement because, partly, it led to increasing social isolation for the group, and it also precluded the Nazis combining with other non-German fascist groups. Their consequent vulnerability to discrimination only enhanced their hostility to their social environment and generated symptoms of increasing paranoia.

Wagner’s study is very well composed and convincingly argued on the basis of a large amount of archival material and interviews. But as is the case with most studies, it also has its weaknesses. An external shortcoming is the somewhat erratic nature of the index; it does not contain many of the names in the volume. The investigation does not descend to the local governmental level. Interviewing and correspondence with resource persons is very commendable for the historian, but the persons chosen for this purpose ought to represent the relevant opposition as well. Of the nine individuals selected (p. 153), none seems to have been connected with anti-Nazi activities between 1933 and 1939. In any case, the volume is not likely to be the last word on the subject.

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JOHN W. HOLMES. *The Shaping of Peace: Canada and the Search for World Order, 1943–1957*. Volume 2. Toronto: University of Toronto Press. 1982. Pp. x, 441. \$37.50.

The unprecedented, if temporary, influence that Canada exercised in international politics through the latter stages of the Second World War and lasting until the mid-1950s, has attracted extensive treatment at the hands of both memoirists and historians. John W. Holmes, retired from a career in Canada's Department of External Affairs and one of the country's most influential essayists and educators on issues of Canadian foreign policy, denies that the two volumes he has produced are either memoir or professional history. These disclaimers should not be taken too seriously: this second volume, like the first, combines many of the virtues of political memoir and careful historical scholarship by offering a treatment that builds on personal experience and a thorough familiarity with scholarly literature (a good deal of it in the form of as-yet-unpublished Ph.D. theses by former students). On certain topics Holmes has used well his privileged access to government records not yet open to scholars, and he also has worked in a good deal of information based on interviews of former colleagues and political leaders. The result is a highly readable contribution to our understanding of recent Canadian foreign policy that will appeal both to those with a general interest in the subject and to scholars.

Holmes's particular focus is on the "set of political ideas," perceptions, and conceptions held by Canadian officials as they shaped and directed Canada's part in the postwar reconstruction. Whereas the first volume dealt largely with vision and hopes, the second addresses itself to revision, disillusion, and adjustment to a world governed not by the ideals of liberal internationalism but by the harsher realities of the Cold War, the nuclear arms race, and the turbulence of decolonization and liberation struggles.

In portraying the revision of Canadian perceptions and policies, Holmes locates his analysis largely within the relationships and institutions that determined in large measure the constraints and opportunities found by Canadian leaders. Each of the major dimensions of Canada's evolution as a middle power is treated in turn: the continental relationship, NATO, the new Commonwealth, the United Nations, the increasing involvement in Asia, and the relationship with the Soviet Union. Holmes's story is concerned largely with demonstrating how the professionals in External Affairs, nearly all dedicated liberal internationalists but divided between pragmatists like Hume Wrong and Norman Robertson and enthusiasts like Escott Reid and Lester Pearson, molded Canadian policies so as simultaneously to protect Canadian national interests and maximize the effectiveness of international institutions in furthering stability and peace in a dangerous world. Given the experience of the Cold War and especially

the education in realism induced by the Korean War—realism concerning America as much as the communist powers—the initial liberal enthusiasm for universal collective security was translated into collective defense in NATO and attraction to mediatory and peacekeeping roles. If the latter would bring the glories of Pearson's Suez diplomacy, it also brought on the tribulations of Canada's role in the International Control Commissions in Southeast Asia.

Holmes's volumes present as effective an apologia as the elite in Canadian External Affairs will receive. His former colleagues are shown as highly skilled, dedicated professionals with their own conception of Canada's national status and role, and a conception of the interests of the Western alliance often more sophisticated than that followed by Americans bedeviled by dogmatic anticommunism or British statesmen ensnared by vestigial colonialism. It was a conception that necessitated an activist diplomacy, particularly through the United Nations, and that had to be defended against parochialism and primitivism in Ottawa and the provinces, as well as in Washington.

Not all readers will be completely convinced by Holmes's treatment—particularly those revisionists and nationalists whose criticisms are given short shrift—but the style and tone, discreet, assured, and professional, are worthy of those whose record he defends.

GEORGE EGERTON

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LATIN AMERICA

DAVID R. MURRAY. *Odious Commerce: Britain, Spain, and the Abolition of the Cuban Slave Trade*. (Cambridge Latin American Studies, number 37.) New York: Cambridge University Press. 1980. Pp. xi, 423. \$44.50.

In *Odious Commerce* David R. Murray traces in exhaustive, and sometimes exhausting, detail the diplomatic history of Britain's long campaign to end the Cuban slave trade. This traffic had begun in 1511 but grew dramatically only with the burgeoning of the island's sugar estates. From Cuban sources, Murray believes some three hundred thousand slaves were imported between 1790 and 1820 (significantly more than Philip Curtin's estimate in *The Atlantic Slave Trade: A Census*). Demand was at its peak therefore, when in 1807 Britain, hitherto heavily engaged in slaving, outlawed the traffic, to the detriment of its own West Indian colonies, and embarked on its "crusade" to get other powers to follow suit. Murray examines this famous British

volte face and its impact on Anglo-Spanish relations.

For Spain the issue was simple. Cuban prosperity depended upon slave labor. The Spanish government could not afford to alienate the island's merchants and planters, who were heavily involved in slaving. This was particularly true after the loss of the mainland colonies, when Cuba, as the richest of Spain's remaining possessions, provided vital revenues. What to the British was a humanitarian crusade to end an iniquitous traffic, was to the Spanish and Cubans an attempt to ruin a rival colony.

Murray weaves his way dexterously through six decades of British exhortation, cajolery, bribery, and often hollow threats, which were met by Spanish stonewalling, excuses, evasions, and half-measures. Concessions won on paper—the outlawing of the Spanish slave trade in 1820 and the right of British cruisers to search Spanish ships—were thwarted by the ever-changing evasion techniques of the slavers, by the refusal of the United States and France to allow Britain to police their flags, by the machinations of the planters, and by the corruption of Spanish officials.

The outlines of the story have long been known; but Murray's account sheds new light on the independent policies of some Spanish Captains-General, on the inflammatory actions of British humanitarians, and on the constraints faced by the protagonists. Thus the Spanish were haunted by the specter, on the one hand, of a slave revolt stirred up by British abolitionists and, on the other, of a white Cuban rebellion prompted by American annexationists. British policy makers were torn between the demands of militant humanitarians and the fear that too rigorous an anti-slave-trade policy would damage their commercial interests in Cuba as well as their relations with Spain in Europe and with America and France in the Caribbean. The United States was alarmed by the annexationist aims of Southerners trying to create a Caribbean empire. White Cubans wanted more slave labor but feared their growing, servile black population. To complicate this already tangled web, Britain and the United States suspected each other of imperialist designs, and Spain feared them both. Small wonder that the policies of all parties vacillated. British efforts waxed and waned to the gain of the slavers, whose profits were so high that in 1861 they could afford to lose one cargo in four and pay enormous bribes to Spanish officials. The losers, of course, were the slaves. Even those who were "freed" from captured slave ships were re-enslaved or, worse still, continually reassigned to new masters as "apprentices" for a lucrative fee pocketed by the Spanish government.

The traffic finally died out only after the outbreak of the Civil War brought American cooperation and led Spain to decide that, with American slavery doomed, it must sacrifice the slave trade in order to

try to preserve slavery itself in Cuba. The last attested slave landing was in 1867.

Solidly based on research in the British and Spanish archives, this book is a valuable contribution to the diplomatic history of the suppression of the slave traffic. While it offers no radical reinterpretations, it rounds out the story by adding useful information, particularly on the aims of Britain and Spain, the treatment of freed slaves, and the methods of slavers. Its narrow focus, however, makes it a work for the specialist.

SUZANNE MIERS
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AMY BUSHNELL. *The King's Coffer: Proprietors of the Spanish Florida Treasury, 1565-1702*. Gainesville: University Presses of Florida. 1981. Pp. ix, 198. \$20.00.

GEOFFREY J. WALKER. *Spanish Politics and Imperial Trade, 1700-1789*. Bloomington: Indiana University Press. 1979. Pp. xvii, 297. \$17.50.

These two works, so different in many respects, are bound together by one salient similarity. Dealing with topics eminently suited to quantification, both authors eschew that path almost completely, presenting their readers instead with treatments of startling orthodoxy. In the process, they fail to elucidate essential elements of their subjects, wasting opportunities within their reach. Despite this failing, however, the books under review deal with important areas of Spanish colonial life and each constitutes a significant advance in our state of knowledge. As such, they can only be welcomed by specialists in the field.

This welcome will be particularly warm for *The King's Coffer*, Amy Bushnell's pioneer study of treasury officials in Spanish Florida. The work is peculiarly interesting from a methodological perspective. In recent years a number of American scholars have come forward with books, dissertations, and articles on colonial treasury (*Caja*) operations. As of yet, however, no clear model for this type of research has emerged for, whereas some of these works merely elucidate bureaucratic history, others have used treasury records to come to a new understanding of economic or political life. *The King's Coffer*, despite its administrative sounding title, is a fine example of social history, and as such constitutes a further development of the possibilities inherent in treasury studies.

Amy Bushnell deals with the proprietors of treasury offices (*oficiales reales*) between 1565 and 1702. After a scene-setting introduction, the author deals with the life style of these officials. The core of the book, however, follows an overtly administrative scheme. Successive chapters discuss the paths that

made the officials wealthy, the organizational structure within which they functioned, the Mexican fiscal transfers (*situado*) that alone made their treasury viable, and the nature of local revenues. The work ends with a discussion of the political functions of treasury officials, a treatment of auditing procedures, and a conclusion. Yet, despite this organization, the most compelling characteristic of the work is its description of the relations treasury officials established with the rest of Florida society. Bushnell's evidence in this regard is invariably well chosen, revealing her finely honed understanding of colonial mores and Habsburg mentality alike. It would be unfortunate if this book were to be regarded merely as a contribution to Florida history, for it has much to teach the Spanish American colonial historian.

The work, of course, is not without blemish. The description of the *Camara de Indias* as "the Executive committee of the Council of the Indies" (p. 34), for example, is inexplicable. More seriously, as the author deals with treasury officials, it is unfortunate that she made no attempt to create a time series out of their audited accounts. At the very least, this would have enabled her to construct a chronology of Floridian economic and administrative life, providing thereby a time dimension that is sadly lacking in this historical work.

Geoffrey J. Walker's *Spanish Politics and Imperial Trade, 1700–1789* partakes some of the same sort of problems. It is peculiar that a work dealing with imperial trade should not be firmly based on a systematic reconstitution of the statistics of colonial commerce. Yet Walker's use of figures, despite his appendixes, never rises above the sporadic. In addition, the title is quite misleading as coverage does not go much beyond 1739. The oddest features, however, are that this 1979 book makes no use of Antonio Garcia Baquero's two-volume 1976 study *Cádiz y el Atlántico, 1717–1778*, and that this Indiana University Press work should not cite G. R. Dilg's fine 1975 Indiana University doctoral thesis, "The Collapse of the Portobelo Fairs." The use of both would have considerably improved the book under review.

Despite such failings, however, Walker's work is a valuable contribution as it is the first full-length English-language treatment of Spanish imperial trade during the neglected first half of the eighteenth century. The treatment is generally chronological, the author focusing particularly on policy discussions and the misfortunes that befell the various convoys sent to the Americas up to the outbreak of the War of Jenkin's Ear. Although detailed, the book alters neither our image of successive French and English threats to Spanish monopoly nor our impression of the tone and tenor of Madrid discussions on the subject. It is to be

hoped, however, that it will focus further attention on this highly important area.

JACQUES A. BARBIER
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VASANT KUMAR BAWA. *Latin American Integration*. Atlantic Highlands, N.J.: Humanities Press. 1980. Pp. xx, 244. \$13.00.

This concise analysis of frustrated Latin American attempts to construct common market organizations is written from the unusual perspective of a senior fellow in the Indian Council of Social Science Research. Vasant Kumar Bawa thus joins the growing ranks of Third World intellectuals learning from each others' experiences. In particular, this book draws on the ideas of the United Nations' Economic Commission for Latin America (ECLA) and its leader, Raúl Prebisch.

Following a brief but thoughtful survey of the intellectual and economic background to attempts at regional integration and self-reliance in the Americas, Bawa probes the history of those efforts since World War II. He examines all such experiments but concentrates on the Latin American Free Trade Area. In his view, these alliances would have been more successful with greater emphasis on subregional collaboration and on joint construction of the infrastructure required for increased commerce within the zone. Above all, he faults the Latin Americans for imitating European examples rather than designing indigenous models of integration derived from the need to overcome exploitation by external powers and internal oligarchies. Based on those criticisms, comparisons are then made with endeavors at economic cooperation among Asian countries.

Although much of the material on Latin American common market attempts is scarcely new, the author's treatment of political forces propelling and encumbering those efforts is refreshingly insightful. His sensitive approach clarifies subtle interconnections and interactions between national actors within each of the Latin American countries—politicians, technocrats, industrialists—and agents on the international stage—ECLA, the United States, Europe, and poorer as well as richer Latin American governments. Not only did foreign policies grow out of diverse domestic political visions and issues within the separate republics but also those internal disputes and decisions were reciprocally shaped by outside influences, be they transnational intellectuals or corporations. However compelling the economic arguments for regional unification, the process was undermined by political conflicts within and among the Latin American nations and by the intrusion of the United States and multinational businesses.

Since the primary beneficiaries of these integration projects so far have been external industrial giants, the wealthier nations within the hemisphere, and the privileged elites inside those countries, Bawa concludes with a negative assessment of achievements to date. Even if the long-lived dream of true political and economic unification to serve the developmental needs of the masses cannot soon be realized, however, he suggests that some multinational groupings—notably the Andean Pact—may at least be able to hammer out mutually beneficial arrangements on functional questions, such as a common currency or the transfer of technology. Bawa closes with the hope that the failed efforts at solidarity have nonetheless raised consciousness, concerns, and concepts that could lead to future collective reductions in dependence and poverty, in Asia as well as Latin America.

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BURR CARTWRIGHT BRUNDAGE. *The Phoenix of the Western World: Quetzalcoatl and the Sky Religion*. (Civilization of the American Indian, number 160.) Norman: University of Oklahoma Press. 1982. Pp. xvi, 349. \$17.50.

The protean figure of Quetzalcoatl, god, priest-ruler, prophet, pervades the mythology and history of ancient Mexico. The legend that he would one day return to claim his lost kingdom most likely contributed to Moctezuma's swift submission to Cortés. Still later, Quetzalcoatl, transformed into a missionary who centuries before had laid the foundations of Christianity in Mexico, became an instrument of nationalistic creoles who sought to establish New Spain's spiritual equality or even superiority vis-à-vis Spain. A "being of rainbow colors," in the words of Burr Cartwright Brundage, Quetzalcoatl's many shapes and baffling transformations challenge scholars to explain the relationships among his many avatars, to discover the "essential" Quetzalcoatl.

The argument of this book may be briefly stated. The author finds four major categories in Aztec religious thought. In the order of their probable first appearance these were the religion of fire; that of earth; that of sky, identified with Quetzalcoatl; and that of Tezcatlipoca. Conceding that these four "religions" mingled with and borrowed elements and gods from each other, Brundage nevertheless insists that "the concentric circles of godhead forming outward from Quetzalcoatl were in toto the picture of a complete religion and that the Feathered Serpent was a center point—or better still the

symbol of an undifferentiated numen at the center point. . . . And this religion, however much it may have touched earth, fire, and the underworld, remained always, and centrally, celestial." This sky religion first appeared among the Olmecs and was transmitted to the Mayas, who developed it further; it "arose to a commanding position in the halcyon days of Teotihuacan and Xochicalco, and was slowly retreating in importance at the time of the Spanish Conquest." Basic to this Mesoamerican sky religion was the concept of the sky as a dragon or two dragons. From this concept derived all the celestial and atmospheric gods. Telluric or earthly concepts, however, were excluded from this sky religion and formed an entirely separate religion that embodied different attitudes toward supernatural power. The bulk of the book seeks to elaborate and demonstrate the validity of this interpretation.

The effort cannot be said to be completely successful. Brundage himself recognizes the apparent exceptions that defy his categories (the Earth Mother, for example, is also the Milky Way and the moon in various of her avatars), and his attempts to explain away these exceptions are not wholly convincing. Much of his reconstruction rests on speculative assumptions based on little or no evidence. Qualifying phrases like "let us guess," "it is possible," "it is probable," abound in the text. A typical line reads: "We are guessing that Quetzalcoatl came originally from the lowlands along the Gulf of Mexico, and we have arbitrarily given his name to the Olmec dragon" (p. 50). There is much reliance on artistic and archaeological evidence to establish mythological correlations, a risky process in the absence of other evidence, since an artistic motif (the Plumed Serpent, for example) could have different mythological significance in different times and places. I am also troubled by Brundage's failure to cite or even refer in his bibliography to such relevant recent works as Nigel Davies's *The Toltecs* (1977) and Karl W. Luckert's *Olmec Religion* (1976).

A final comment on Brundage's treatment of Topiltzin Quetzalcoatl, the priest ruler of Tula, as opposed to the god—Brundage offers the traditional version, based on Sahagún and other sources, that Topiltzin was a reformer who forbade the sacrifice of human victims and substituted for it the offering of birds, butterflies, and snakes. Since the same sources, however, depict the god Quetzalcoatl as devoted to human sacrifice, I am inclined to share the skeptical view of other scholars who suggest that Topiltzin's supposed opposition to the practice was an embellishment by post-Conquest Indians interested in making one of their major gods acceptable to the Spanish conquerors.

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MANUEL MIÑO GRIJALVA *et al.* *Tres aspectos de la presencia española en México durante el porfiriato: Relaciones económicas, comerciantes y población.* (Centro de Estudios Históricos.) Mexico City: El Colegio de Mexico. 1981. Pp. ix, 235.

This study of the economic presence of Spain and of Spaniards in Mexican life during the period from 1876 to 1911 is the product of a graduate seminar in history at the Colegio de México. Written by three graduate students, the work illustrates the revival of interest in things Spanish that has accompanied Mexico's recent (1977) resumption of diplomatic relations with Spain following the death of General Francisco Franco, whose regime the Mexicans never recognized. Two of the three essays in the book, all of which are based on extensive research in Mexican archives, focus on Spanish immigrants and the role they played in the Mexican economy. The other essay deals with trade between the two countries.

With respect to this trade, Manuel Miño demonstrates that although both countries remained underdeveloped their commerce was complementary. Mexico's exports to Spain consisted mainly of primary products, such as chickpeas, henequen, and dyewoods; Spain exported to Mexico principally semifinished and manufactured products, for example, cigarette paper, wines and liquors, and olive oil. The balance of trade favored Spain by a considerable margin throughout the period studied, and despite the growth of both nations' economies, and a substantial increase in the volume of their trade with each other, commerce between Mexico and Spain remained of small significance quantitatively when placed in the perspective of each country's overall foreign trade. Established trading patterns with industrial powers, Mexico with the United States, Spain with England and France, grew more pronounced, even as the Mexico-Spain commerce was expanding, and the development of a Mexican economic infrastructure during these years only served to strengthen commercial ties between Mexico and its northern neighbor.

Most Spanish immigrants to Mexico were single males who came as—or soon after arrival became—businessmen. Although the influx produced a significant growth in the size of the Spanish colony, from just over six thousand to almost thirty thousand, the total immigration was relatively small by Western Hemisphere standards. Still, Spanish immigrants did play an important economic role in the era. Heavily concentrated in two commercial centers, Mexico City and Veracruz, they came to dominate retail trade. The most successful of them, moreover, expanded into such areas as banking, industry, and agriculture, and through their investments helped facilitate and accelerate Mexico's economic development. In this respect, Pedro Pérez

notes, Spanish immigration constituted one of the few positive aspects of a Mexican "colonization" policy that largely failed. Despite being the largest foreign group resident in Mexico, however, Spaniards contributed only modestly to total foreign investment in the Mexican economy. They ranked sixth in this respect, and French investment, which ranked fifth, was eight times greater than Spanish. Most of the immigrant businessmen from Spain, then, remained petty retail merchants who probably stimulated far more Hispanophobia among the Mexicans than economic expansion in Mexico itself.

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HARRY E. CROSS and JAMES A. SANDOS. *Across the Border: Rural Development in Mexico and Recent Migration to the United States.* Berkeley: Institute of Governmental Studies, University of California, Berkeley. 1981. Pp. xix, 198. \$12.00.

This book is an attempt to synthesize social science research on twentieth-century Mexican migration to the United States. It provides an overview of the root causes of emigration and the complementary pull forces that give direction to displaced people from rural areas. The authors, Harry E. Cross and James A. Sandos, also present their recommendations for policy changes and further research.

Here is a needed corrective for the overly legalistic approach of American policymakers, who usually fail to recognize the function of economic change and demographic growth. Since the start of the Revolution of 1910, Mexico's north central states have suffered from severe developmental imbalances. The destruction caused by various rebellions prevented many rural workers from earning a living. The post-World War II "green revolution" brought only selective modernization and ignored the needs of the rural poor. Large commercial farms received the lion's share of government credit and technical aid. Farmers cultivating small plots of maize—already living at a subsistence level—got little assistance. Their out-migration, because of increasing population, was inevitable.

Mexico cannot accommodate these displaced people. Its Gross Domestic Product in 1975, with a population of 57 million, was \$71 billion. In the same year, the greater Los Angeles area, with a population of 10 million, had a gross product of \$81 billion. Only emigration to the much richer United States can provide the safety valve needed for political stability; it is the accepted and expected response to Mexico's rather dismal employment situation. Eagerly awaiting Mexico's displaced work-

ers are American employers, especially those in California's service and light industries.

This book provides a short, handy synopsis of the dynamics of Mexican immigration. Although the authors do not examine all recent literature that bears on the subject, they have touched base with many important studies. Their conclusions emphasize how much we do not know about this population movement. They note that no adequate data exist for determining how many migrants are in this country, how many decide to stay here and why, and their impact on the wage and employment structure of the host country. The authors would like to see a strengthened border patrol, a system of worker identification cards, and a five-year program that would admit qualified migrants and allow for the collection of data needed by policymakers. I doubt that this approach would be favored by the Mexican government, since it would entail a public admission that Mexico cannot care for its own. One can hear Mexican nationalists objecting to the use of their fellow citizens as "guinea pigs" for Uncle Sam and American academics. Strengthening of the border patrol and instituting identification cards have not proven to be politically feasible in the past. There is little reason to think they would be so now.

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MAURIZIO VERNASSA. *Emigrazione, diplomazia e cannoniere: L'intervento italiano in Venezuela, 1902-1903*. Leghorn: Editrice Stella. 1980. Pp. 152.

This tightly organized little monograph on Italy's participation in the Anglo-German blockade of Venezuela evidently owes its inspiration to Alberto Aquarone's massive work on the origins of American imperialism, in which Italy's role in Venezuela receives a passing and tantalizing mention. (A. Aquarone, *Le origini dell'imperialismo americano* [1973], pp. 352-54). Maurizio Vernassa combines standard, archivally documented diplomatic narrative with a close look at the social and economic reasons for Italy's involvement in the Caribbean republic, and in so doing he thoughtfully eschews all temptation to apply the "pre-Fascist" label.

Vernassa's analysis leads us in quite a different direction. Italy appeared in Venezuela as an exporter of human labor-power, commercial enterprise, and engineering skills. Unlike Great Britain, Italy had no interest in backing one caudillo against another in the endless civil wars of Venezuela. No big Italian bank was inclined to match the German Diskontogesellschaft's scale of investments—and losses—in Venezuela. Unlike these two truly imperialist powers, Italy could put forward a genuinely human motive for its intervention: the plight of the

Italian immigrant worker, trapped in a jungle of intrigue, violence, and robbery.

Almost equally pitiable was the plight of the Italian Foreign Ministry in Rome and the Royal Italian Legation at Caracas. Although Italy was the southern anchor of the Triple Alliance, Germany showed no concern whatever to let the government in Rome know about its plans for blockading and bombarding the Venezuelan coast. The German minister in Caracas, like his British colleague, refrained from cooperating with the Italian minister, Riva, whose anguished dispatches form the principal part of Vernassa's documentary appendix. At first, neither London nor Berlin showed any inclination to put Italian creditors and plaintiffs on the same footing as their own citizen claimants. The Italian foreign minister, Prinetti, had to mobilize all the resources and arguments at his command in order to ensure that Italy might eventually participate on a basis of formal juridical and military parity with the Anglo-Germans in the intervention against Cipriano Castro's regime in Venezuela. Vernassa believes that Italy received this shoddy treatment, initially, because both London and Berlin were apprehensive about how the United States might react to the inclusion of a third partner in their punitive operations. His narrative also brings out the pathetic combination of assertiveness and deference with which Italy advanced its claims to consideration as a great power. The Italian minister at Caracas lagged behind his British and German colleagues in breaking relations with Venezuela and entrusting Italy's interests to the Americans. In a like manner, the Italian naval commander in Venezuelan waters had to put up with the overbearing attitude of the German Commodore Scheder, who tried to countermand the orders that the Italians had received from the British admiral in command of the whole international fleet.

Although the Italians were on the other side of the barricades, both their diplomats and their newspapers were in a good position to understand the anger of Venezuela's Castro, who declared to Minister Riva on May 6, 1902, that at the head of his two million citizens he could make war on "todo el mundo." When the whole tragicomic episode ended with American-sponsored arbitration, no party was more satisfied than the Italian government.

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JONATHAN KELLEY and HERBERT S. KLEIN. *Revolution and the Rebirth of Inequality: A Theory Applied to the National Revolution in Bolivia*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xvi, 279. \$26.50.

This is an interesting and, in many ways, provocative book. Jonathan Kelley and Herbert S. Klein have formulated a "theory" (set of empirically testable propositions) regarding the impact of revolutions on socioeconomic equality in the contemporary world. This theory is then tested against data drawn from postrevolutionary Bolivia.

Hence, this is not a study of the Bolivian revolution per se; the book, therefore, contains no new narrative analysis regarding this least studied of modern revolutions. Students of Bolivian affairs will not go away empty handed, however. Kelley and Klein do present a rich array of hitherto unpublished socioeconomic data on the revolution's winners and losers and give thereby a picture of the new lines of social stratification emerging in Bolivia in the mid-1960s.

The data are drawn primarily from a very important project conducted in Bolivia in the mid-1960s by the Research Institute for the Study of Man (RISM). Aside from some preliminary reports by members of the RISM team, this book is, as far as I know, the first systematic and theoretically informed attempt to mine those data. For this reason alone, the study is significant; and it confirms the importance of the original project and the richness of the data that remain to be worked on by social scientists.

This book gives a good picture of the socioeconomic impact of the revolution of 1952. It shows that the revolution wrought its biggest change in the agricultural sector. Here the big winners from the revolution were the mass of Indian peasants, who in concrete ways were liberated from an oppressive, serflike position in prerevolutionary Bolivia. The peasants' gains, not unexpectedly, were at the expense of the large landowners, who were the big losers in the immediate aftermath of the revolution. However, Kelley and Klein also show that the initial trend toward greater equality, manifested in the gains of peasants and other groups, later gave way to a postrevolutionary pattern of inequality that presumably continues to solidify into a new, and far from egalitarian, stratification system. In addition to these socioeconomic data, there are some very interesting data on popular perceptions of the revolution.

Space does not permit a discussion of the study's methodological and theoretical foundations, which are in many ways the most important aspect of the book. Suffice it to note that these aspects of the book should draw some substantial challenge from a variety of points of view.

In very simplified terms, the theory of the book is that modern revolutions transform socioeconomic systems and, in the short term, promote equality; but such equalization soon gives way to a new pattern of inequality; and this pattern is then per-

petuated by the ability of some to transmit privilege from generation to generation. There are a variety of potential questions regarding the theory, including some tautological aspects to the first step in the theory.

The most controversial point should prove to be, however, the causal mechanism that Kelley and Klein posit to explain the broader structural shifts. The driving force behind this mechanism is human capital (talent, training, and skill), the basis upon which a new elite merges. The concept of human capital and its use as the master key of explanation should be good for some very heated polemics. In sum, this is a book that deserves to be read, studied, and disputed by those interested in Bolivia and the process of modern revolutions.

JAMES M. MALLOY
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DAVID GUEIROS VIEIRA. *O protestantismo, a maçonaria e a questão religiosa no Brasil* [Protestantism, Freemasonry, and the Religious Question in Brazil]. Foreword by GILBERTO FREIRE. (Coleção Temas Brasileiros.) Brasília: Editora Universidade de Brasília. 1980. Pp. 409.

The so-called "Religious Question," or the "Bishops' Conflict," which embroiled church-state relations in Brazil during the early 1870s, led, in 1875, to a diplomatic retreat by the Vatican's Foreign Office that amounted to a sweeping victory for regalism and a defeat for the relatively weak Brazilian Catholic church. The issue brought larger questions to the fore than simply the ostensible right of fraternal Masons to continue to associate with lay Catholic brotherhoods—historically part of the Brazilian experience—or of priests to remain Masons as long as their clerical duties were not impeded. The clash, which led to arrest, trial, and imprisonment of two Brazilian bishops, pitted the church's newly elaborated ultramontane orthodoxy against the secularized monarchical state headed by Dom Pedro II, who saw nothing wrong, as the titular head of the Brazilian church, in being a member of the Masonic Order. Classic nineteenth-century liberalism embodied in Brazil's conservative governing elite, led in 1889 to the fall of the empire itself and the promulgation of a highly decentralized *laissez-faire* republican system that survived to the near-exclusive benefit of the prosperous states of the Center-South until it was pulled down by a military coup in 1930 seeking a return to nationalistic centralization.

David Gueiros Vieira, a historian on the staff of the state of Pará's Emílio Goeldi Museum, has produced a detailed study of a generally overlooked factor in the religious conflict: the influence of Protestant missions, especially in the northern Ama-

zon. This is virtually the entire focus of the book. Protestants, tolerated in Brazil because of the (mostly unfulfilled) hope of luring Germanic and Anglo-Saxon immigrants, fought against the ultramontane demands of the Vatican and acted, according to the author, as "the catalytic agent which precipitated the clash between Church and State."

While Gueiros Vieira does offer considerable detail to show the extent to which such men as Aureliano Cândido Tavares Bastos, Ashbel Green Simonton, and Richard Holden worked through existing political networks to further their case for religious freedom of expression, his static form of description and his failure to examine larger themes raise considerable doubt about the aggregate impact of the Protestants, always a mostly foreign group and never more than a small minority even within the elite. The book's "history through biography" approach ignores the larger fabric of Brazilian life and shies away from any significant consideration of the implications or the consequences of its topic. The author is to be commended for producing a massive compendium of primary and secondary sources, mission reports, newspapers, letters, and archival documents gleaned from repositories in Brazil, the United States, Great Britain, and the Vatican. The study's value lies in its description of Brazilian Protestantism, less so as a major contribution to our understanding of the church-state dynamics as a contributing factor in the developing secularization of Brazilian society.

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GERTRUDE MATYOKA YEAGER. *Barros Arana's Historia jeneral de Chile: Politics, History, and National Identity*. (Monograph Series in History and Culture.) Fort Worth: Texas Christian University Press. 1981. Pp. xiv, 187. \$12.00.

Gertrude Matyoka Yeager has chosen to write about one of Chile's leading nineteenth-century intellectuals: Diego Barros Arana, whose career included not only serving his nation as a legislator and diplomat but also as director of the country's most prestigious secondary school as well as dean of the University of Chile's Faculty of Philosophy and History. Despite these rather awesome administrative duties and his dedication to public service, Barros Arana managed to write a multivolume history of Chile plus numerous monographs, textbooks, and scores of newspaper articles.

Approximately a third of Yeager's book describes the family of Barros Arana, the scholar's early life, his academic career as well as his achievements in the fields of public administration and diplomacy. Throughout his life, Barros Arana was a devoted

Liberal and his commitment to his party led to his dismissal from the post of minister of education. Despite this reversal, he continued to advocate state-funded secular education and to favor other Liberal causes. He also served in the chamber of deputies where he opposed personalist government. Predictably, he sided with the Congressional forces during the 1891 revolution that deposed José Manuel Balmaceda. Barros Arana remained active until the end of this life, in 1906.

The remainder of Yeager's monograph analyzes the historian's epic study, the *Histoire jeneral de Chile*, as well as a two-volume work describing the regime of General Manuel Bulnes (1841–1851). Unlike many of his contemporaries, Barros Arana based his efforts on primary sources, which he used in order to demonstrate that Liberalism was not new to Chile but that it had taken root during the colonial period. Consequently, the historian broke with the current Liberal fashion by praising preindependence Chile as a dynamic society that, he argued, slowly managed to reform its basic institutions, culminating eventually, in the struggle for freedom from Spanish rule. Barros Arana, however, did not consider the colonial epoch unflawed, but he tended to attribute most of the nation's problems to the influence of the Habsburgs and the Roman Catholic church.

Barros Arana also differed from other historians, arguing that O'Higgins represented the culmination of Chilean Liberalism while dismissing José Miguel Carrera as a self-serving caudillo. Unlike his political colleagues, Barros praised the Liberals' traditional *bête noire*—Diego Portales—arguing that he restored order, which, by stabilizing Chile, allowed the nation to become more stable and to progress. Barros Arana's study of the Bulnes administration sought to contrast the relatively benign rule of the general with that of his successor, Manuel Montt (1851–1861), a man whom the historian and many Liberals loathed.

While not without merit, Yeager's book suffers from a variety of serious flaws. She tends to describe Barros Arana and his work in a most uncritical fashion. She never, for example, explores fully the historian's feud with Montt. The author, moreover, fails to put Barros Arana and his work into some historical perspective as did Julio César Jobet's *Temas historicas chilenas*. The biographical material is disappointing because it rarely includes primary sources and because it tends to skip large portions of Barros Arana's career. The bibliography, which tends to outdistance the material actually cited in the footnotes, fails to include seminal works like Blakemore's study of the 1891 revolution or the Guman-Vio study on the Tavoré episode. Numerous factual errors seriously detract from the work. The author confuses President Francisco Antonio Pinto

with his son Anibal, has the archbishop of Santiago dying four years after the fact, and fails to describe adequately either the Pinto regime (1876–81) or the events precipitating the 1891 revolution. The author's analysis of the *Historia jeneral*, moreover, tends to be a summary rather than a critical effort. In short, Yeager's efforts do not fulfill the promise of her introduction.

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HEINRICH VOLBERG. *Auslandsdeutschtum und Drittes Reich: Der Fall Argentinien*. Cologne: Böhlau. 1981. Pp. xiv, 219. DM 48.

As a salesman for the German Chemical Trust, young Heinrich Volberg emigrated to Argentina in 1928. In the 1930s he became head of the Nazi party's Economic Office in Argentina, where one of his principal duties was to eliminate Jewish influences from the German business and industrial establishments in that country. In 1939 he was one of the party's Foreign Organization officials attached, over the diplomats' objections, to the German Embassy in Buenos Aires. During the war his activities were the subject of several inquiries by Argentine officials. In 1944, after Argentina and Germany broke relations, he was a member of the official party repatriated to Portugal. He has now, at the urging of Professor Günther Kahle, written a memoir of the secret war in Argentina. He draws upon few written sources, but he has performed the invaluable service of looking up contemporaries in Argentina and Germany (some of whom have since died) and recording their testimony. Apart from interrogations and affidavits taken from German participants during the immediate postwar years, this is the only memoir to offer evidence from the Axis side on great-power rivalry in the La Plata region during the 1930s and 1940s—a subject on which the murk has begun to lift only in the last few years. Sadly, however, the book belongs to the "*Ach wie war es damals schön!*"—the "How-lovely-it-was-then!"—school of German Third Reich historiography and is of limited usefulness.

To be sure, Volberg makes some solid points. He is quite justified in protesting the persecution of German pioneer settlers in Misiones Territory. (One of the most celebrated incidents, the discovery of the Great Nazi Arms Cache near the town of Apóstoles, was in fact a hoax engineered by two Argentine newsmen who borrowed antique weapons from a local museum for the purpose.) He is right to denounce the clumsily forged documents, produced

in 1939, which purported to show that Germany had sinister designs on the vast, nearly empty land of Patagonia. He is correct in impugning the motives of Raúl Damonte Taborda, a political chameleon who by the end of the war had earned the hatred of the U.S. Embassy, whose protégé he had once been. He is also correct in challenging the evidence published by Damonte's Parliamentary Committee on Anti-Argentine Activities (much of it produced by Heinrich Jürges, a tireless forger responsible also for the Patagonia documents and numerous other tarradiddles). Unquestionably, the way the Allies waged the secret war in Argentina was fierce and dirty.

But Volberg's memory is highly selective. Asserting that Germans in Argentina in the 1930s had no notion of the extent of Nazi persecution of the Jews, or of the possible outcome, the author laments at length the injuries wrought to German trade by the Jewish boycott. He fails to note the Nazis' great and enduring contribution, in funds and materials, to *criollo* antisemitism. He is correct in saying that German schools were less often examined in Buenos Aires Province than in federal jurisdictions. Can he have forgotten that this was because Buenos Aires was then governed by Manuel Fresco, an aspiring fascist caudillo supported by the German Embassy? (Fresco later scored the politician's equivalent of the hat trick by going on the British payroll as well.) It simply strains credulity to claim that, following the government's crackdown of May 1939, the German associations, including the NSDAP, were converted to the letter and democratic spirit of the new regulations. Volberg grows lyrical at the "unity" forged among overseas Germans by the Third Reich. He passes over in silence the party's sordid and divisive harassment of *Volksbund* president Dr. Röhmer or of Ambassador Thermann. He has nothing to say either of the recruitment of *Volks-* and *Reichsdeutsche* for Germany's clandestine services in wartime. To have done so would have necessitated discussion of the Hellmuth mission, a hilarious fiasco that provided the Allies the pretext they needed to force Argentina's diplomatic break with Berlin early in 1944. In a larger sense, it seems clear that although Germany's agents in Argentina accomplished little to further the Axis war effort, they did much to invite persecution of the German-Argentine communities that perforce remained behind when the exchange ships went steaming off with their precious diplomatic cargo in mid-1944. But the author misses this dimension entirely.

One would like to say, with archy the cockroach, "Yours for less justice and more mercy." But sometimes—even after forty years—it still isn't easy.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JACK W. MEILAND and MICHAEL KRAUSZ, editors. *Relativism: Cognitive and Moral*. Notre Dame, Ind.: University of Notre Dame Press. 1982. Pp. x, 260. Cloth \$20.00, paper \$8.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I would like to clarify or correct several statements Professor J. H. M. Salmon made of his review of my *Representative Government in Early Modern France* in the *AHR* (87 [1982]: 183–84).

I never said, nor do I believe, that the provincial estates were "the determining element in French government in the fifteenth and sixteenth centuries." Rather they were only one type of institution in which the people participated. Towns, villages, and the duly constituted bodies involved more people, but I chose "the provincial estates as my central theme" and treated the other "popular" institutions in a summary fashion (p. 2 and elsewhere).

Charles V destroyed the provincial estates. Only the estates of Dauphiné (which was technically not part of France) and the estates of a few fiefs that were convoked by their feudal lords (for example, Burgundy) met between ca. 1390 and 1420. If this fact is accepted, one must admit that the provincial estates grew in importance after 1420. The role of the villages in government also increased, more towns won charters, and the older towns held their own.

The crown often created offices to sell and in its edicts sometimes gave the need for money as the excuse for doing so. Therefore, if the crown readily suppressed new offices at the request of the estates in return for financial compensation, it is logical to assume that it was more interested in money than in the new officials. For example, the crown created *élus* in Agenais time and again during the sixteenth century only to suppress them at the request of the estates. When the crown again created *élus* in Janu-

ary 1603, the leaders of the estates were once more prepared to offer money for their suppression. Sully, however, tried to prevent the estates from meeting and its deputy from seeing Henry IV and his council for fear that Agenais's offer would prove too tempting for his master and colleagues to resist. On several occasions he is reported as saying that he planned to treat the other provincial estates in a similar fashion, and he took some steps to do so. Hence, Sully, with Henry IV's blessing, inaugurated a new policy, a policy of substituting royal tax collectors for those of the estates, of capturing the tax collecting machinery. Marillac returned to this policy in the 1620s. Under these circumstances I do not think that it is "special pleading" to differentiate between the early Bourbon creation of *élus* with the creations of their Valois predecessors.

I would like to express my gratification that Professor Salmon accepted my argument that Marillac was more "the architect of absolute monarchy" than Richelieu. To my knowledge he is the first leading authority to do so.

J. RUSSELL MAJOR
Emory University

TO THE EDITOR:

Congratulations to the *AHR* for publishing the provocative discussion of "Western Civ" (*AHR*, 87 [1982]: 695–743). My only regret is the way many historians concede the obsolescence of Western Civ, just as a few years ago some historians accepted the irrelevance of history itself.

As one who has taught Western Civ every year for almost twenty-five years (without TAs), I can say that all of us in the profession have profited enormously from the inventions of the founders, but we certainly do not have to be bound by their limitations. One can abandon the Whig assumptions of an earlier age, and one can accept the charges that Western civilization has been racist, sexist, and imperialist, and still find value in the Western Civ course. For better or for worse, "the rise of the

West" is still the dominant phenomenon in the history of mankind.

Besides acknowledging this central fact, a Western Civ course can also be one of the sort splendidly described by Professor Woehrlin, one that "contributes significantly to a student's interest in and sense of the past, that develops those critical and analytical skills that are so much a part of historical study, and that teaches a student to ask questions about how human communities function and change. . . ."

Any historian should be proud to teach such a Western Civ course, and it deserves a place in any curriculum.

JOHN C. MOORE
Hofstra University

TO THE EDITOR:

Fear of race, sex, and religion clouds the "AHR Forum: 'The Rise and Fall of the Western Civilization Course'" (*AHR*, 87 [1982]: 695–743). Implicit rejection characterizes each of six *Forum* references to religion. Black history, as such, is passed over. Women are "the gender group" (p. 728). Let history rather shine with: courage in the face of race and self-identity; acceptance of the earthiness of human sexuality along with a cerebral awareness of the niceties of grammar; humility before religion and the finitude of existence. On these points, clarity is essential for the post–World War II student body.

RAYMOND J. JIRRAAN
Thomas Nelson Community College

TO THE EDITOR:

This past May, introductory history students from across the nation took a common Western Civ exam. The writing section required first an hour-long essay on standard fare—for example, explanation of Weimar's collapse, similarities and differences between nineteenth century socialism and liberalism, and continuities and changes in the policies of Nicholas II and Stalin. Students freely chose one question from among six offerings on the narrative sweep of European history from 1450 to the present. The second essay, however, was obligatory. It was based on documents selected and edited for this particular exercise in historical analysis and synthesis. The 1982 document-based question (DBQ) dealt with continuities and changes in upper-class modes of child-rearing in preindustrial England. *Not* your traditional history by a long shot.

What's the point? Frankly, I am amazed at omission of the Advanced Placement European History (APEH) program in the *Forum* discussion, "The Rise and Fall of the Western Civilization Course,"

(*AHR*, 87 [1982]: 695–743). Overlooking the academic merits of a Western Civ course growing at a 15 percent annual rate is an inexcusable instance of ivory tower isolation from dynamic trends in undergraduate education taking place *off* college campuses—in over a thousand public and private secondary schools.

Although the conventional college survey course may have suffered from malaise and confusion in the seventies, APEH has experienced reinvigoration and new direction. Thirty-seven hundred candidates took the national exam in 1972; a decade later, their number was pushing ten thousand. One reason for the phenomenal increase may lie in cost savings. A forty-two dollar exam fee can earn the equivalent of 6–8 semester hours and placement in advanced-level history courses. I suspect, however, that heightened student interest has more to do with the quality of AP teachers and their creative syllabi.

AP teachers are a unique lot. They are survey specialists who value their AP teaching assignment. Just recently, one Utah teacher confessed to me that he would have quit public school teaching years ago if it had not been for AP. AP is viewed as the cream of the secondary school teaching schedule in spite of the fact that its teachers must juggle the dual roles of exalted lecturer and over-burdened teaching assistant. Even with the brightest and best mix of students, AP teachers must stimulate interest, orchestrate divergent discussion, and evaluate an unstoppable flow of essays and papers. Furthermore, they must keep abreast of historical developments and research findings to up-date curriculum and prepare candidates for the national exam.

The College Board, which administers the program, helps with local conferences and AP teacher-consultants. And the national exams furnish leads and teaching tools as well. For example, the DBQ, described earlier, has introduced teachers to recent trends in scholarship. Since the mid-seventies, DBQs have focused on such issues as seventeenth-century witchcraft persecution, women's education from the Renaissance to the Enlightenment, middle-class and working-class work behavior and work attitudes in industrializing Europe, and nineteenth-century demographic trends and reactions to them in France and Germany. Thus, previous years' DBQs provide practice exercises for working with primary source documents and furnish social history materials to supplement traditional textbook accounts.

Unfortunately, the 1970s AHA sessions on Western Civ and the invitational Annapolis Conference failed to recruit some of the most dynamic survey specialists in the field—the AP teachers—for insight and inspiration. A well-balanced AP course includes both history as content and history as methodology. It integrates the history of elites with the experi-

ences of ordinary people. It examines historical continuity along with change. And it covers the so-called "big" topics, but more often with a new slant alongside the old, thus reflecting the concerns of social historians. Lastly, it undergoes refinement and change as AP teachers redraft their courses to reflect new interests and developing strengths, whether in art history or gender roles.

Since students' essays are read holistically at the June national reading by college and secondary school teachers—Are the students' arguments cogent? Is the thesis defensible? Are the supporting data relevant?—AP teachers have flexibility to design their courses creatively within broad thematic frameworks.

I suspect that Allardyce *et al.* have unwittingly ignored the emerging victors on their freshman turf—the APEH teachers. Too bad. Allies need all the help they can get. And it's high time survey teachers in secondary schools and academia joined forces to mutually develop the commodious historical perspective an educated citizenry requires for the political and personal choices our era demands.

MILDRED ALPERN

*Spring Valley Senior High School
Spring Valley, New York*

TO THE EDITOR:

Two additional points bear mentioning in the welcome attention being given to the introductory history course, the first point methodological and the second substantive (*AHR*, 87 [1982]: 695–743).

None of the writers of the *Forum* can enthrall any one, and very few historians are even faintly aware of the need to reach the general public, the general reader, or the freshman. The descent into professionalism has been so frequently pointed out that it hardly needs any further elaboration, and yet little seems to be done about it. Historians, with a few notable exceptions like William H. McNeill, seem unable to take a world view and are fearful of commitment to a value system; so the aptitude for synthesis is waning. In short, historians seem to have little to say to the general reader, and the current discussion seems so far to have been conducted by professionals, for professionals, and in a professional manner. Of course, the plague of specialization and interest in techniques has cut off from the general public not only history but also most "serious" literature, music, and art. Perhaps one of the ways of escape from this methodological trap is for historians to think of themselves as writers rather than as researchers.

The substantive point is equally interesting. All of the contributors, with the possible exception of William F. Woehrlin, seem to think of the sixties not

only as the period of the demise of Western Civ but also as a disaster area in general. They see only revolt and fragmentation, where there were new directions and new capacity for enthrallment and dedication. Historians certainly did not speak to the condition of the sixties, but others did, for example, Rachel Carson, Carlos Castaneda, Ram Dass, E. F. Schumacher, to name a few. The generation of the sixties and the young of today managed to muster three quarters of a million people in Central Park to demonstrate for peace—a significant statement of dedicated enthrallment.

The "greening of America," which began in the sixties is an affirmation of the spiritual over the material. Finally, materialistic determinism, whether of the Marxist or merely the pedestrian sociological variety, has run out of steam; and it is notable that most of the new drives have a religious source, whether Christian, Hindu, Buddhist, or Muslim. This development dates McNeill, but so far the new spiritualism does not seem to have found its voice among historians.

Perhaps a beginning can be made by taking up the traditions of the old idealist historians like Croce, Collingwood, Meinecke or Burkhardt. Naturally we cannot today share their values any more, but we can try to emulate their style.

GEORGE T. PECK
Bowdoin College

TO THE EDITOR:

Every person concerned with general survey courses for college freshmen, and particularly with the general survey of history called Western or World Civilization should read the article in the *American Historical Review* for June 1982 (pp. 695–743), entitled "The Rise and Fall of the Western Civilization Course." It is by a series of experts discussing experiments at Columbia, Harvard, the University of Chicago, Amherst, and Stanford between 1900 and 1970.

Courses in Western Civ were made popular for several decades by professors like James Harvey Robinson and Charles A. Beard. They had a core content centering upon the position of Western powers like England, France, and Germany, but declined and decayed as a result of the fall of Western nations in World Wars I and II. Attempts to keep them going and to revive them have not been very successful, and the majority of teachers have concluded, perhaps, that world history courses are too boundless and too simple for a complex world where specialization is the rule. It would seem that history is no longer relevant to the modern world.

I would agree that the course, as usually taught, is

dead, because it is rotten at the core and must be changed. I think I know how to change it.

I used the Columbia study of Contemporary Civilization in the middle 1920s when I taught at Emporia Kansas State Teachers College, and the Harvard system of teaching in the late 1920s at Ohio State University. I also taught the course at Ohio Wesleyan, and for many years at Utah State between 1933 and 1971.

By the time I retired in 1971 my course had changed a great deal from what it had been in 1924 when I started teaching. As a college student I had fallen in love with history through the textbooks of Robinson. Among my great teachers in graduate school at Chicago were Andrew C. McLaughlin, William E. Dodd, Bernadotte Schmitt, Louis Gottschalk, Ferdinand Schevill, and Marcus Jernegan, four of them presidents of the American Historical Association. I was a colleague of Homer Hockett, Carl Wittke, and A. C. Cole at Ohio State. With forty-nine years of college teaching the Civ course and ten years of trying to write a text for it, I feel that I can speak with some authority and confidence.

My friend, William H. McNeill, speaks of irrelevance and antiquarianism as the central failure of our profession in the last two decades. "The only thing that can rescue us from such a fate," he says, "is to find something worth teaching to undergraduates *en masse*, something all educated persons should know, something every *active* citizen ought to be familiar with in order to conduct his life well and perform his public duties effectively" (*AHR*, 87 [1982]: 696).

The central core of a course in world civilization—the modern world of today and of the future—should be science. Neither the specialist in science nor the specialist in history is qualified to write it. While agreeing with a *Science* editor (1883–1885) who wrote: "Science must be almost as much popularized to be made accessible to all scientific readers, as to be read by the educated public who were never in a laboratory," I also agree with Frederick Jackson Turner, one of the first presidents of the American Historical Association, who said "A comprehension of the United States of today, an understanding of the rise and progress of the forces which have made it what it is, *demands* that we should rewrite our history from the new points of view afforded by the present."

The world today is the world of the atom, the integrated circuit, and the silicon chip. Should not such a course on world civilization deal with those things that are making the world into a single, interdependent community? There is no question that the most underdeveloped people on the earth want the same things that the most developed country on the earth also want, for with these things comes the power to rule. The most ruthless dictator

is after the same technological tools wielded by the most humane and "peace-loving" president or ruler of one of the great powers. Power lies in the control of oil, the control of food, the control of rubber, and the control of weapons. Commentator Bill Moyers has compared the world of David Rockefeller to the power of the English crown in the last century, and to the papacy in the Middle Ages. The "World of David Rockefeller" is an international banking world that deals with friend and foe alike, loaning billions of dollars to both communistic and capitalistic nations. The world of the grain merchants and of the makers of guns is much the same.

The ruling classes of today live in a world economy. As yet the average citizen is unconscious of their power, though terms like ITT, IBM, NASA, DNA, and RNA are coming into his lexicography. He uses the tools of the new technology but does not know what they mean to his life or to his future—or to his country's future. A freshman course should teach the significance of the rise of the conglomerates and the multinationals. It should emphasize world perspectives and seek to give a world outlook to the young thinker.

This world viewpoint will have no place for the old nationalism and the old mythologies. Terms like "national sovereignty," "freedom of the seas," "sovereignty," and "independence" are meaningless in a world economy where a great power, such as the United States at present, is dependent upon others for most of its strategic metals and oil. It will be difficult to accept Einstein's dictum that "The unleashed power of the atom has changed everything but our modes of thinking, and thus we drift to unparalleled catastrophe." This task of rethinking history will be most difficult for historians. In the old jargon a Plymouth or a Dodge made by Mitsubishi-Chrysler may be as difficult to our thinking as a Spanish-American with the right of social security north of the border, or a pipeline carrying gas from Siberia to the industrial heart of West Germany as something either beneficial or harmful. Viewpoint may be everything, and perspective may be determined by the place whereon we stand.

Yet there was a glimmer of light among orthodox thinkers when the Council of the American Historical Association in 1981 decided to consider the publication of a popular history magazine. (See its *Newsletter*, [September, 1981]: 4.) The magazine was to "redefine the frontiers of the discipline, include essays of synthesis, overview and interpretation; articles dealing with similarities and contrasts over space and time." It was to be popular, entertaining, and relevant. It was to encourage generalizing and storytelling, and "would permit the specialist to speak to someone other than another specialist."

Aye! There's the rub; can we speak it so freshmen can understand it, and like it? We can and they will; provided first, that we take a world perspective,

second, make science the core, and third, write with a journalistic flair. But it will not be easy!

J. DUNCAN BRITE
Pasadena, California

TO THE EDITOR:

I was much interested in the article, "The Rise and Fall of the Western Civilization Course," in the June 1982 issue of *AHR*. It occurred to me that my experience in a similar course a number of years ago might be helpful.

Soon after I joined the faculty of Loyola College, Baltimore, in 1947, it was decided by authorities that all freshmen should be required to take a general survey course in history. My colleague and I chose a text that was approved by the dean and the president. On the morning of the first classes, we were informed by the dean that a Jesuit priest who was recovering from a nervous breakdown and who had asked to have one section of the course in an effort to get back to work, had found great fault with the selected text. He had combed the volume and submitted six pages containing words, phrases, and parts of sentences that he regarded as anti-Catholic. We were told the text could not be used.

I immediately set to work and compiled my own notes, from which I taught for three years. The course—which soon came to be known as "From the Ape Man to 1960," the date 1960 (then a decade in the future) allowing for two weeks of discussion of problems facing us in the 1950s—was the most satisfying course I ever taught. I attempted to cover at least briefly every significant event, movement, and person; and we touched on every part of the world.

I was assigned two large sections, and when the priest collapsed again at Thanksgiving, I inherited his section and found I had to start at the beginning. I remember that I had one section of 157 students, including all science students, all business students, all non-Catholic students, and all of the few black students then enrolled.

I was gratified later to have alumni, then state and city officials, lawyers and judges, doctors and dentists, business men, tell me that they had found the survey course the most valuable of any they took. They discovered that when one of their professors in graduate school mentioned a name or an event, they knew right away what the man was talking about, and they had a jump on their fellow students who had never heard of those things. One brief example: the law students recognized Hammurabi and Justinian and their codes of law and could make the connection with their current work, while their colleagues were often in the dark and had to look up the references.

WILLIAM B. HOYT
Rockport, Massachusetts

TO THE EDITOR:

I write to comment on a statement made during the exchange between Professor Gilbert Allardyce and Professor Carolyn Lougee in the June 1982 issue of the *AHR*, concerning the history and current status of Western Civilization programs in the United States.

In Professor Lougee's response to Allardyce's essay, she refers to his having demonstrated that the "five paradigmatic [Western Civ] programs did disintegrate" over time, one of which, I gather, would be the University of Chicago Western Civilization course. Western Civ at Chicago is alive and well. After a comprehensive review of the undergraduate curriculum last year, the social sciences faculty in the college voted enthusiastically to continue to require the three-quarter Western Civilization course as part of a structured program in civilizational studies that each undergraduate must take. Students at Chicago take, typically, the Western Civilization sequence, a parallel sequence in one of a number of non-Western Civilizational courses taught each year in the Chicago college (East Asian, South Asian, African, Islamic, Latin American, and Russian), and a final, integrative civilizational course that seeks to offer a more global and comparative perspective in which the student can move from intra- to inter-civilizational analysis.

The Chicago Western Civ course still emphasizes the political, intellectual, and cultural history of Europe; it still employs an empirical, topic-oriented approach utilizing an extensive range of primary sources; it still enjoys great popularity among our students. The staff of the course includes, aside from historians, members of the departments of classics, education, and sociology and the school of law. Although we constantly revise our nine-volume series, *Selected Readings in the History of Western Civilization*, to reflect serious new historiographical developments, including in recent years materials on the history of women in the Middle Ages and on the cultural interaction of Europeans with non-European peoples (such as blacks and Indians), the Chicago staff has refused to bow to each particularist interest movement that has swept across the contemporary historical profession.

JOHN BOYER
University of Chicago

TO THE EDITOR:

I have just read Professor Allardyce's article on the Western Civilization course in the June 1982 issue of the *American Historical Review*. It was stimulating and insightful, but both the article and the respondents have missed several points. They seem to be in the ivory tower of higher education, the private liberal arts college. I write as a teacher of Western Civiliza-

tion (among other things) in a small California community college, where the course is still alive and well.

First, Professor Allardyce has not cast a wide enough net. The "Western Civilization" course of which he writes is not the course for which so many textbooks have been written. Ask the publishers who buys their Western Civ textbooks and how the courses are taught and you will find standard three-hour-per-week lecture courses taught by historians all over the country, mainly in community colleges. For years the Western Civilization course at Monterey Peninsula College has been healthier than the course on which it was modeled at San Jose State College. (We also have a team-taught, interdisciplinary course on the Allardyce model, called GEN-TRAIN.)

Second, Western Civilization has never been the "first year of the college course" in the public colleges and universities of California, and I think that is true of the nation as a whole as well. That course is and always has been the year course in American history. Even in my graduate years at the University of California (Berkeley) European history majors had to "TA" in American history if they wanted employment (that was back in 1957 to 1960).

Third, he should have tied in his article with the demise of foreign-language requirements all over the country.

Fourth, the lack of defense of Western Civilization by history departments needs further explanation. I believe that history departments will go the way of classics departments unless they realize (1) that the initial survey courses are indispensable sources of students for the upper division, (2) that, as such, far more thought needs to be put into bringing them up to date, (3) that no history department can prosper in a vacuum, (4) that no survey or

general education course can survive if it is not required, and (5) that other disciplines must be made to see the importance of Western Civilization to their majors if such a course is to have students.

In the race for specialization we are creating a crop of semiliterate surgeons and space technicians.

I personally believe that the "Western Tradition" is still the core of society in the late twentieth century, and that he or she who is not familiar at least with the broad patterns of the European cultural heritage will be handicapped in the world of tomorrow. The non-Western world is in the process of abandoning its past in favor of "Western" technology and "Western" values. The Japanese are producing baseball players and concert musicians. The Africans are producing Olympic athletes and oil for automobiles. The Latin Americans produce United Nations General Secretaries and sink British ships with French missiles.

In designing any general introductory course one should keep in mind that for many (if not most) students it will be the last history course ever taken. With that in mind, "a general history history [must be] free of 'every unessential detail which serves only to obscure the great issues and transformation of the past,' " the Zen Buddhists notwithstanding. If it is not chronological the students will be without a major organizational tool in an age when the multiplication of knowledge requires organization.

History is a very useful tool for the college graduate, and historians have a professional obligation to fight for its inclusion in the curriculum. If we retire to the world wherein some students learn almost everything about almost nothing we will abandon future generations to the "moral majority," the Pentagon, the scientists, and Wall Street.

C. POHL HAMMER
Monterey Peninsula College

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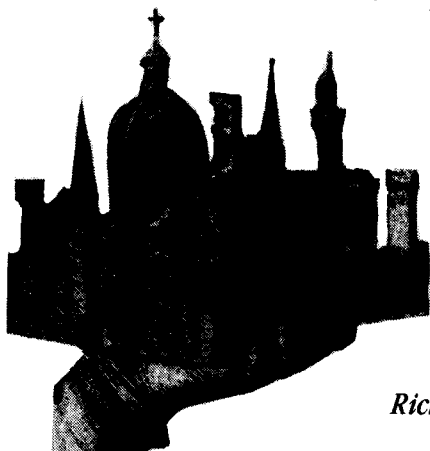
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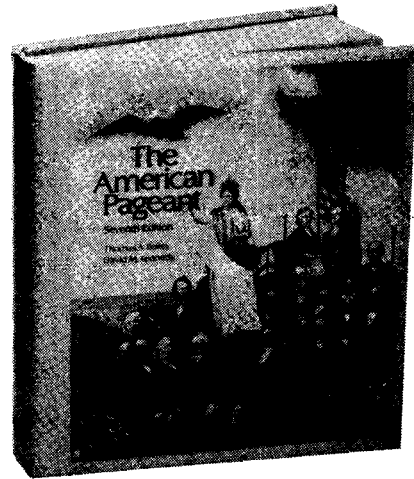
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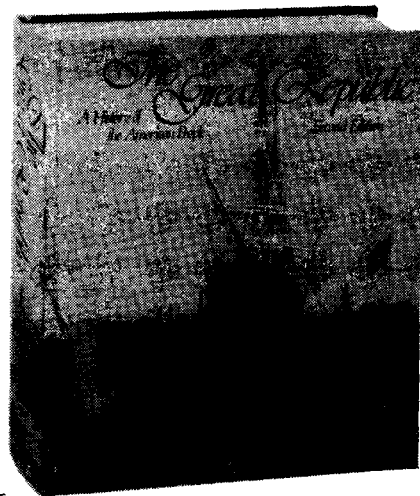
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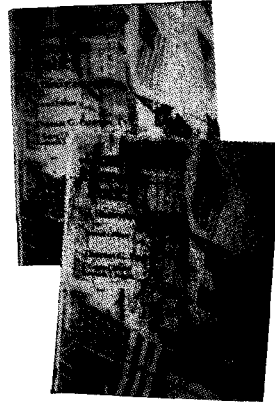
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